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## PALEONTOLOGY

### A REVIEW OF SOME SUBGENERIC NOMENCLATURE AMONG THE *LEPIDOCYCLINAE*

(TERTIARY ORBITOIDAL FORAMINIFERA)

BY

T. F. GRIMSDALE AND I. M. VAN DER VLERK

(Communicated at the meeting of November 29, 1958)

The revision of such a group as the *Lepidocyclinae* naturally involves a review of the status of each generic and subgeneric name ever formally proposed for them, and their status must be assessed in two ways, nomenclatorially and taxonomically. This note considers the nomenclatorial and taxonomic status of *some* of them, its object being to determine which names are valid, available for use if required, and which invalid or synonyms, and for what reasons<sup>1)</sup>.

Early preliminary action is desirable in some of these cases, because the unexpected conclusions reached may excite critical opposition, which may render necessary decisions by the International Commission on Zoological Nomenclature for their settlement.

#### *The type species of Nephrolepidina* H. DOUVILLÉ, 1911

In 1911 appeared HENRI DOUVILLÉ's paper on Tertiary Foraminifera from the Philippine Islands, wherein are the first enunciations of two of the published subgeneric names for *Lepidocyclina* GÜMBEL, 1868, namely *Eulepidina* and *Nephrolepidina*. DOUVILLÉ's actual words will here be quoted, since besides being the first published definitions of these two subgenera, they pose a problem:

“Dans un mémoire précédent et à l'exemple de MM. VERBEEK et FENNEMA, j'ai divisé ce genre en deux sections, pour lesquelles j'ai proposé les noms suivants:

1. Les *Eulepidina* généralement de grande taille, caractérisées par leur loges équatoriales en spatule ou en hexagone subrégulier et par leur nucleus du type embrassant.

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<sup>1)</sup> Before handing in this review for printing we submitted the manuscript to two experts on zoological nomenclature, Dr. H. BOSCHMA, professor of Zoology at the University of Leiden, and Dr. L. B. HOLTHUIS, curator at the State Museum of Natural History at Leiden. We owe many thanks to both gentlemen for their criticism.



2. Les *Nephrolepidina*, presque toujours petites, à loges équatoriales ogivales, en losange ou en hexagone allongé et à nucleus du type réniforme" (H. DOUVILLÉ, 1911, p. 59).

In the first line of the foregoing quotation, DOUVILLÉ has mentioned "un mémoire précédent . . .". To what memoir is he referring? It cannot be to any of his previously published works, since this 1911 paper is famous as bearing the earliest mention of the two subgeneric names. He must therefore have been speaking of an account unpublished at that date, which either remains unpublished, or the publication of which was delayed until later.

In our opinion (and it will remain no more than an opinion unless unexpected additional evidence is encountered) the "mémoire précédent" to which he refers is his paper on the Foraminifera from Nias Island, published in the following year (H. DOUVILLÉ, January 1912). It is true that in the 1912 paper he again speaks of "un mémoire précédent", this time clearly in reference to the 1911 "Philippine" publication; but this may simply be an editorial insertion, at the moment of publication, in a work written prior to the Philippine memoir. For this opinion, the internal evidence is quite consistent, since the 1912 Nias paper gives careful diagnoses of the two subgenera—superior in our view to those in the Philippine memoir. Moreover, in the 1912 paper, he comes very near to designating type species for both subgenera. Again we will quote his words:

(*Eulepidina*) "Dans un premier groupe celui de *L. dilatata* . . .". While this is probably the basis for YABE's designation of *Lepidocyclina dilatata* as the type species of *Eulepidina* (YABE, 1919, p. 41), it is insufficient foundation for a case to be made out in favour of DOUVILLÉ himself as designator. The situation for *Nephrolepidina* is less uncertain, and DOUVILLÉ's intention is obvious, for he wrote as follows:

"Les *Nephrolepidina*, de taille plus petite que les précédentes, présentent à peu près toujours des pustules dont la disposition est caractéristique.

"La forme fondamentale est *L. verbeeki*, établie par NEWTON et HOLLAND pour l'*Orb. papyracea* DE BRADY. Le type est un échantillon recueilli par VERBEEK, sur les hauts plateaux de Padang (Sumatra) . . ." (DOUVILLÉ, 1912, p. 269).

DOUVILLÉ's reference here to "le type . . ." must unhappily be construed as relating to the type specimen of the species *Lepidocyclina verbeeki* and not to the type species of the subgenus *Nephrolepidina*. Nonetheless his intention to consider *L. verbeeki* the type is—in our view—perfectly clear. On the following page he adds concerning *L. verbeeki*;—"Cette espèce représente dans l'Extrême Orient le groupe européen du *L. marginata*, et en particulier *L. tournoueri*" (H. DOUVILLÉ, op. cit. 1912, p. 270).

Possibly on the ground of this statement YABE later designated *L.*

*marginata* (MICHELOTTI) = *Nummulites marginata* MICHELOTTI, 1841 as the type species of *Nephrolepidina* (YABE, 1919, p. 41); his rather casual attitude in so doing suggests that he in no sense realised at the time that he was making a formal designation, but rather that he was merely voicing DOUVILLÉ's own view. By the rules of zoological nomenclature we are compelled to accept YABE's designation.

In one respect this is a singularly unfortunate choice. *Lepidocyclus marginata* is a microspheric form, whose megalospheric partner is not established beyond all doubt <sup>1</sup>). The three major text-books in the English language all commence their account of the subgenus *Nephrolepidina* by describing the properties of the megalospheric nucleocoenonch, a feature which is unobservable in the type species! For this situation two remedies exist; one, to confine the definition of *Nephrolepidina* to properties which may be observed in the microspheric type, the other, to prove from topotype material which megalospheric species corresponds to the microspheric *L. marginata*. (See CUSHMAN, 1948, Chapter on *Orbitoididae*, by VAUGHAN and COLE revised by COLE; p. 362; GALLOWAY, 1933, p. 436; GLAESSNER, 1945, p. 168).

*The type species of Amphilepidina* H. DOUVILLÉ, 1922

The type species of *Amphilepidina* now accepted is *Lepidocyclus sumatrensis* (H. B. BRADY) = *Orbitoides sumatrensis* H. B. BRADY, 1875. In his own opinion, J. J. GALLOWAY designated this species as type in 1928 (see GALLOWAY, 1928, 1933). However, he was preceded in this by H. DOUVILLÉ who, in 1925, wrote — under the heading “sous-genre *Amphilepidina*” — as follows: “*L. sumatrensis* peut être considéré comme le type de ce sous-genre” (H. DOUVILLÉ, 1925, p. 100).

There remains one faint source of doubt to be dispelled. In 1924 VAUGHAN had already designated another species as the type, and we will quote his actual words: “H. DOUVILLÉ has proposed the subgeneric name *Amphilepidina* for species with nephrolepidine embryonic chambers and spatulate meridional chambers. The type species is *Lepidocyclus chaperi* LEM. and R. DOUV.” (VAUGHAN, 1924, p. 795). *Lepidocyclus chaperi* was not in the original list of species assigned to *Amphilepidina* by DOUVILLÉ himself in 1922 when he proposed the subgenus; but in 1924, and prior to VAUGHAN's designation, DOUVILLÉ had already added *Lepidocyclus chaperi* and two further species to his subgenus in the first part of his “Revision des Lépidocyclines” (H. DOUVILLÉ, 1924, pp. 44–46). This work VAUGHAN had before him when he designated *L. chaperi* as the type species of *Amphilepidina*; in proof of this we have

<sup>1</sup>) If there be indeed certainty that *Lepidocyclus tournoueri* is the megalospheric form of *L. marginata*, then the name *tournoueri* is redundant, and both partners should be known as *L. marginata*. The retention of *L. tournoueri* seems to indicate a lack of assurance that the two forms are correctly paired.



the fact that (a) DOUVILLÉ's 1924 "Revision des Lépidocyclines" appears in VAUGHAN's list of "publications cited (VAUGHAN, 1924, p. 815); (b) the existence of internal evidence in VAUGHAN's paper, viz., inclusion of *Amphilepidina persimilis* DOUVILLÉ, 1924 in VAUGHAN's list of species (VAUGHAN, 1924, p. 798), and his mention of DOUVILLÉ's assignment of *Helicolepidina spiralis* TOBLER, 1922 to *Spiroclypeus* (H. DOUVILLÉ, 1924, p. 21; cited by VAUGHAN, 1924, p. 802).

Though it may seem logical to accept DOUVILLÉ's later inclusion of *Lepidocyclina chaperi* in his subgenus as tantamount to its presence in the original list of species, the strict rules of zoological nomenclature leave no room for doubt on this point. *Lepidocyclina chaperi* was not in fact mentioned in DOUVILLÉ's 1922 list of species, but *L. sumatrensis* was. Hence it follows that *L. sumatrensis* must continue to be accepted as the type species of *Amphilepidina* (Rules, art. 30, e,  $\alpha$ ).

*The validity of Polyorbitoina VAN DE GEYN and VAN DER VLERK, 1935 and of Isorbitoina THALMANN, 1938; the invalidity of Orbitoina VAN DE GEYN and VAN DER VLERK, 1935 and of Pliorbitoina VAN DE GEYN and VAN DER VLERK, 1935*

In 1935 VAN DE GEYN and VAN DER VLERK founded a new genus *Orbitoina*, characterized by the four stolons connecting each equatorial chamber with those adjacent to it. There was no definite, unambiguous designation of a type species by the authors, nor has any been offered subsequently. The name *Orbitoina* is therefore invalid.

In the same work, three subgenera were proposed (for the genus *Orbitoina*); namely *Isorbitoina*, *Pliorbitoina*, and *Polyorbitoina*; in no case was a type species designated. However *Polyorbitoina*, being monotypic (sole species *Lepidocyclina (Polylepidina) proteiformis* VAUGHAN, 1924), is nomenclatorially valid (Rules of Zoological Nomenclature Art. 30, 1c). The same monotypy also applies to *Pliorbitoina*, but this name is an objective synonym of *Pliolepidina* H. DOUVILLÉ 1915, since both have *Lepidocyclina tobleri* H. DOUVILLÉ, 1917 for type species, *Pliolepidina* having priority in time.

*Isorbitoina* when originally proposed was not monotypic, and the name remained invalid until THALMANN (1938, p. 202) designated *Lepidocyclina (Lepidocyclina) trinitatis* H. DOUVILLÉ, 1924 as its type species. Although THALMANN referred to the article of VAN DE GEYN and VAN DER VLERK (op. cit. 1935) for diagnosis of the subgenus, *Isorbitoina* must be ascribed to THALMANN (1938, p. 202) and not to its original proposers, and it remains nomenclatorially valid.

*The validity of Pliolepidina H. DOUVILLÉ, 1915*

The subgenus *Pliolepidina* was proposed by H. DOUVILLÉ in 1915 for an unnamed species of *Lepidocyclina* (H. DOUVILLÉ, 1915, p. 727, fig. 34).

In 1917 this species was named by DOUVILLÉ himself *Lepidocyclus* (*Pliolepidina*) *tobleri*, and it has rightly been accepted as the type of *Pliolepidina* until 1940. We are therefore correct in surmising that the name *Pliolepidina* H. DOUVILLÉ, for a subgenus of *Lepidocyclus*, type species *Lepidocyclus* (*Pliolepidina*) *tobleri* H. DOUVILLÉ (1917, p. 844, fig. 5, 6) is nomenclatorially valid (Rules of Zoological Nomenclature, art. 30, 1c).

In 1928 GALLOWAY (1928, p. 63) elevated *Pliolepidina* to generic rank, a status which he maintained in his "Manual of Foraminifera" (1933, p. 432). No other authors have admitted this.

Later VAUGHAN and COLE (1940, p. 322; 1941, pp. 64–66) have emended the definition of *Pliolepidina* upon the assumption that *Lepidocyclus tobleri* is no more than a teratologic form of *Lepidocyclus pustulosa* H. DOUVILLÉ (1917, p. 844, fig. 1–4), substituting the latter species for *L. tobleri* as the type.

BRÖNNIMANN reflects upon the subjective nature of their view concerning teratology (op. cit., 1946, p. 376), and further points out that it is not permissible to perform such a substitution; if *Lepidocyclus tobleri* is not a valid species in its own right, then *Pliolepidina* becomes a "nomen caducum". This attitude is reinforced by the Rules of Zoological Nomenclature, art. 30, II, e,  $\alpha$ , since *Lepidocyclus pustulosa* was not included under the name *Pliolepidina* at the time of its publication, and so can never become its type species.

COLE (1948, p. 361) responds to this challenge by placing *Neolepidina* in the synonymy of *Pliolepidina*, attempting to overcome the nomenclatorial impasse by making *Lepidocyclus pustulosa* forma *tobleri* H. DOUVILLÉ forma teratologica the type of *Pliolepidina*. In this way he retains the name *tobleri* in the redesignation, but in a sort of varietal status only.

Unless the Rules of Zoological Nomenclature are to be reinterpreted to allow of this anomaly, we consider that it is impossible for a subspecies, or variety, or forma, of a species, other than one bearing the same trivial name as the species, to be accepted as the type of a genus or subgenus.

In this particular instance, the acceptance by COLE of *Lepidocyclus trinitatis* as a synonym of *L. pustulosa* (VAUGHAN and COLE, 1941, p. 65) renders *Pliolepidina* H. DOUVILLÉ, 1915 emend. COLE 1948 a synonym (for him as for us) of *Isorbitoina* THALMANN, 1938, thus causing his whole case to collapse.

#### *Neolepidina* BRÖNNIMANN, 1946

In 1946 BRÖNNIMANN, following a comprehensive review of the subgenus *Pliolepidina*, proposed a new subgenus *Neolepidina* (BRÖNNIMANN, 1946, p. 378), type species *Lepidocyclus pustulosa* H. DOUVILLÉ, 1917. This name is nomenclatorially valid. However, VAUGHAN and COLE have placed *Lepidocyclus trinitatis* H. DOUVILLÉ, 1924 in the synonymy of *L. pustulosa* (VAUGHAN and COLE, 1941, p. 65), a judgement with which



BRÖNNIMANN appears to be (op. cit., 1946, p. 376), and we are, in agreement. *L. trinitatis* is, however, the type species of *Isorbitoina* THALMANN, 1938, as we have already seen. In our opinion, then, *Neolepidina* must fall into the synonymy of *Isorbitoina*.

*The invalidity of Tryblioepidina* VAN DER VLERK, 1928

When the subgenus *Tryblioepidina* was founded in 1928 the author included five species: *L. ruttenii* VAN DER VLERK, 1924, *L. talahabensis* VAN DER VLERK, 1924, *L. orientalis* VAN DER VLERK, 1924, *L. leytenensis* YABE and HANZAWA, 1925, and *L. ephippioides* JONES and CHAPMAN, 1900. No type species was designated.

In several handbooks *L. ruttenii* is indicated as the type. This may be easily understood for it is implied in the founder's notes on this subgenus that he considered *L. ruttenii* to be the type. Nevertheless another species, *L. ephippioides* JONES and CHAPMAN has to be recognized as the type, since W. BERRY (1929) designated this species as such (Rules of zoological nomenclature, art. 30, II g).

Study of the original material of JONES and CHAPMAN at the British Museum (Natural History) in London convinced us that *L. ephippioides* has to be considered as belonging to the subgenus *Eulepidina*. Both VAN DER VLERK (1928) and BERRY (1929) were misled by plate 5, figure 2 in JONES and CHAPMAN's publication. This picture was made of an imperfectly equatorial section. Plate 5, figure 1 gives a clearer picture of the shape of the nucleoconch in horizontal section. It follows that, according to the rules of zoological nomenclature, *Tryblioepidina* is synonymous with *Eulepidina*, so *Tryblioepidina* should be rejected as a subgenus. If it later appears convenient to regard other species which VAN DER VLERK included in *Tryblioepidina* as a group deserving to be elevated to subgeneric rank a new subgenus name will be necessary.

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# EVOLUTION IN THE AMERICAN *LEPIDOCYCLINIDAE* (CAINOZOIC FORAMINIFERA): AN INTERIM REVIEW. I

BY

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(Communicated by Prof. I. M. VAN DER VLIERK at the meeting of Nov. 29, 1958)

## ABSTRACT

The *Lepidocyclinidae* are believed to have had a diphyletic origin, and the two lineages to have pursued separate but convergent courses of evolution from Middle Eocene to Miocene. The first of these, here termed *Lepidocyclina* "Lineage Y", must be named *Lepidocyclina* because it embraces the type species of that genus. The other, termed *Lepidocyclina* "Lineage X", cannot yet be accorded a name, on account of inadequate knowledge of the type species of *Nephrolepidina*, a microspheric species, from which the stolon-system has never been described.

"Lineage Y" evolved from *Tremastegina* via *Helicolepidinoides* and *Eulinderina*; its first representatives are species of the *Lepidocyclina antillea*-group, from which the groups of *L. macdonaldi* and *L. ocalana* arose during the Eocene, both being "isolepidine". From the latter group sprang two sublineages, the *L. chaperi*-group ("nephrolepidine"), in the Upper Eocene, giving rise in the Oligocene to the "eulipidine" species; and the *L. mantelli*-group, whence arose the *L. giraudi* and *L. canellei* groups, all three mainly "isolepidine". However, one or more species of the *L. canellei*-group produce transitional forms to "nephrolepidines" (viz. *L. californica*), which are scarcely distinct from the *L. "tournoueri"*-group ("nephrolepidine") in character.

The earliest representative of "Lineage X" is the group of *Lepidocyclina r. douvillei*, contemporaneous with *L. antillea* in the Middle Eocene; these two species differ markedly from one another, more so than from their supposed relations in the Upper Eocene. It is not easy to envisage them as directly related to one another. From the *L. r. douvillei*-group sprang the Upper Eocene *L. pustulosa*-group, and the aberrant *L. veracruziana*, both "isolepidine". The *L. pustulosa* group also threw off an aberrant species, *L. tobleri*, with a "pliolepidine" nucleocoenoch. From the *L. pustulosa*-group evolved the Oligocene *L. yurnagunensis*-group, with its late Eocene herald *L. subglobosa*. *L. yurnagunensis* produces a range of nucleocoenoch types from "isolepidine" to "nephrolepidine", providing transitions to *L. vaughani*, a "nephrolepidine" species. For the present, "Lineage X" is retained in *Lepidocyclina* s.l., but it is recognized that it merits separate status, subgeneric or generic, when the nomenclatorial difficulties are surmounted.

Further subgeneric classification is also postponed until the equatorial layer and stolon system of all species have been adequately studied. However, it is recognized that, in view of the general mutual resemblance of most of the species of *Lepidocyclina* s.l., an economy of names might be preferable to a complex subgeneric taxonomy.

Lastly, a commensal mode of life with unicellular green algae is seen as a possible



cause of convergent evolution among the large Foraminifera, by favouring the adoption of an annular mode of growth in place of the helical spiral of the ancestral forms.

## INTRODUCTION

The genus *Lepidocyclina* was first elevated to family rank, the *Lepidocyclinidae*, by SCHEFFEN (1932, pp. 251, 252), and four years later TAN SIN HOK sustained its family status, elaborating a reasoned argument for his case (1936, p. 277). This was founded largely on the work of BARKER and GRIMSDALE (1936), wherein it is implied that the Eocene "orbitoidal" species until that time grouped conventionally under the names *Polylepidina* and *Helicolepidina*—originally proposed as subgenera of *Lepidocyclina*—have a phyletic history independent of the true Orbitoididae (mainly of late Cretaceous age), apparently stemming from a genus of "smaller Foraminifera" during early Eocene time.

A considerable number of genera and subgenera have been proposed by various authors—mostly since the year 1910—, which are assignable to the *Lepidocyclinidae*, and no study of the family would be complete without a full analysis of these in order to determine their priority, their validity or otherwise, and to settle which species are to be regarded as their respective genero- and subgenero-types. In order not to interrupt the thread of discussion, this analysis is relegated to Appendix 1. There a total of 27 names will be found, of which only five are actually used in the present study, although many others are mentioned. The five to be used are *Lepidocyclina*, *Eulinderina*, *Helicolepidina*, *Helicolepidinoides*, and *Pseudolepidina*. A similar drastic reduction will be effected among the long list of species proposed, though much of this has already been foreshadowed in published studies by VAUGHAN (1933, 1937), VAUGHAN and COLE (1941), and COLE (1944, 1952, 1953, 1957).

This stringent limitation of the number of genera and subgenera is not, however, necessarily permanent. Its application here is desirable because, in this study, a novel and unorthodox mode of classifying the species of the *Lepidocyclinidae* is adopted which corresponds little with the established subgeneric classification, but which, for the moment, has to be regarded as tentative and incomplete, requiring a far greater volume of information and evidence to be compiled before a proper generic and subgeneric framework can be constructed.

## SUGGESTED POLYPHYLETIC ORIGIN OF THE *LEPIDOCYCLINIDAE*

From long study of the American *Lepidocyclinidae* I have gradually reached the conclusion that at least two distinct lines of descent may be recognized within the genus *Lepidocyclina*, sensu lato, as here interpreted. I was led to this through noting the marked distinctions to be observed between certain contemporaneous species at any stratigraphic level from Middle Eocene to Miocene; whereas more obvious congeners

of each might be found in the immediately adjacent levels above and below. I have endeavoured to express this in Table 1, which is evidently a crude adumbration of the more elaborate "family tree" in Figure 2.

TABLE I

Middle Eocene	r. douvillei-group	antillea-group
		Macdonaldi-group
	pustulosa-group	ocalana-group
Upper Eocene	tobleri	
	veracruziana	chaperi-group
	subglobosa	
	yurnagunensis	mantelli-group
		ephippioides-group
Oligocene		giraudi-group
		canellei-group
		asterodisca
	vaughani	"tournoueri"
	dartoni	

An early intimation that *Lepidocyclus* might not be monogenetic was given by H. DOUVILLÉ (1925, p. 73); — "La dissemblance des deux genres (viz. *Nephrolepidina* and *Eulepidina*) est telle qu'on peut se demander s'il existe entre eux une parenté réelle: elle ne pourrait être en tout cas très éloignée". BARKER and GRIMSDALE have claimed *Lepidocyclus* to be polyphyletic (1936, p. 245) in origin, and DROOGER (1956, p. 463) is of the same opinion, apparently.

The two main lineages now recognized may be traced from Middle Eocene to Oligocene, with Miocene developments of one or of both occurring principally in the Eastern Hemisphere. Whether the later representatives of two lineages can be disentangled amongst the less obviously heterogeneous species groups of the Far East Miocene is at present doubtful; the effects of convergence may have obliterated the more striking characteristics by which the Eocene forms can be distinguished. For the moment, both these lineages are retained within the single comprehensive genus *Lepidocyclus*, though it is reasonable to suppose that, when sufficient information is available to test the theory adequately, they may merit separate generic status. One of these genera must, of course, continue to be named *Lepidocyclus* — that is, the one containing the type species *Lepidocyclus mantelli* (Morton). For the other lineage, an appropriate name will have to be selected from among those available.

In this account, the principal *Lepidocyclus* lineage will be referred to as "Lineage Y"; the other, as yet unnamed, lineage will be termed "Lineage X".

The origin of "Lineage Y" in *Eulinderina* has been postulated by implication in the work of BARKER and GRIMSDALE already cited (1936, p. 245), wherein they show that the species described as *Lepidocyclus*



*chiapasensis* VAUGHAN (= *Lepidocyclus antillea* CUSHMAN, teste COLE, 1944, pp. 57–60), and classed by them as *Polylepidina*, is but a step removed from *Eulinderina semiradiata* BARKER and GRIMSDALE, phylogenetically—that step comprising a slight reduction in the nepionic spiral in the megalosphere together with the loss of the so-called “counter-septum” or everted inner lip of the principal foramen. This everted lip had provided them with the critical clue by which the ancestry of *Eulinderina* could be traced back, via *Helicolepidinoides* (*Helicostegina* in BARKER and GRIMSDALE<sup>1</sup>) to a fully spiral genus of “smaller Foraminifera”, *Tremastegina parvula* (CUSHMAN)<sup>2</sup>, thus removing the necessity for seeking an origin for many of the Tertiary orbitoidal Foraminifera in the late Cretaceous Orbitoididae.

The origin of “Lineage X” cannot yet be identified. Its earliest known representatives, belonging to the group of “species” centring around *Lepidocyclus r. douvillei* LISSON, are found in the middle part of the Middle Eocene (Claiborne equivalent) of Eastern Mexico together with *Lepidocyclus antillea* CUSHMAN, this latter being, of course, the earliest representative of “Lineage Y”. These two species differ from one another so markedly that their common ancestor, if such existed, must be envisaged as remote stratigraphically; whilst, as BARKER and GRIMSDALE have shown, the direct ancestral forms of *Lepidocyclus antillea* can be identified nearby—both stratigraphically and geographically—in the Middle Eocene of the same area.

It may conveniently be recollected here that in *Helicolepidina* a third independent lineage must be acknowledged, which was recognized by TAN (1936) as a separate subfamily, the *Helicolepidininae*—as distinct from the *Lepidocyclininae*. This lineage may be held to stem directly from *Helicolepidinoides*, without the intervention of a *Eulinderina* stage.

#### THE EQUATORIAL LAYER IN *LEPIDOCYCLINA*

The equatorial layer comprises the least unstable group of elements in a plastic group of organisms. This has been positively stated by certain authors, and is tacitly acknowledged by the majority, in that almost all taxonomic and evolutionary studies of the genus have been largely concerned with the equatorial layer, in particular with the nucleocoenoch and the nepionic chambers.

Actually, the equatorial layer may be regarded as comprising four parts:

1) TAN SIN HOK's adjustment (1936, p. 243 et seq., 254) is accepted; and see under *Helicostegina* and *Helicolepidinoides* in Appendix 1.

2) Described by BARKER and GRIMSDALE as *Amphistegina lopeztrigoi* Palmer (BARKER and GRIMSDALE, 1936, p. 233). BRÖNNIMANN (1950, pp. 255, 256) has erected a new genus, *Tremastegina*, to which probably the species *lopeztrigoi* must be referred; while subsequently COLE (1958), p. 201 has placed the species in the synonymy of *Amphistegina parvula* (CUSHMAN), (= *Nummulites parvula* CUSHMAN), which is now found to be an “Amphisteginid”.

- (i) The embryonic apparatus or nucleocoenoch, consisting generally of two chambers, but occasionally of more than two.
- (ii) The periembrionic ring of chambers, termed the nepionic chambers.
- (iii) The chambers which lie outside the periembrionic ring but within the main mass of equatorial chambers, from which they differ to some degree in size and shape: in some species chambers of this type cannot be differentiated.
- (iv) The main mass of equatorial chambers.

All these chambers are interconnected with one another by the so-called stoloniferous passages, making up the stolon system; but reference to the latter generally applies to the stolons linking the main mass of equatorial chambers.

The embryonic apparatus and the periembrionic ring of nepionic chambers have been accorded far more consideration than the rest of the equatorial layer, and will not be described more fully here; reference is made in particular to the published works of TAN SIN HOK (1935, 1939), of RENZ and KÜPPER (1947), and of VAN RAADSHOVEN (1951). Attention will therefore be directed to the main mass of equatorial chambers and their stolon system.

Attempts to make use of the stolon system for taxonomic purposes and as a means of tracing the evolution of the *Lepidocyclines* have not so far proved very successful (VAN DE GEYN and VAN DER VLIERK, 1935; VAUGHAN, 1936; VAUGHAN and COLE, 1940, 1941, 1948; BRÖNNIMANN, 1946). This is partly due to a paucity of information about the stolons in many species, but also to misunderstanding of the probable manner in which the stolon pattern and the chamber pattern are mutually interdependent. I will endeavour to explain this.

Growth in the equatorial layer was annular, the chambers being added in more or less regular concentric rings. However, despite this constant mode of growth, the pattern of the main mass of equatorial chambers does not always clearly display a concentric arrangement. In fact it may exhibit predominantly one of several designs, as seen in equatorial section:

Radial — the chambers are arranged along radial lines: this is best seen in *Lepidocyclina peruviana*.

Intersecting curves, also termed the "engine-turned" pattern: this is best seen in *Lepidocyclina yurnagunensis*, but is widely distributed.

Circular concentric rings, as in *Lepidocyclina canellei*.

Polygonal rings, as in *Lepidocyclina asterodisca*.

In some species no very definite pattern is discernible.

The chambers themselves are generally interiorly rounded, but a geometrical framework is often visually superimposed, as is evinced by the terms often employed in describing chamber shape, viz. hexagonal,



rhombic, diamond shaped. Chambers with hexagonal framework are illustrated diagrammatically in Figures 1a, 1b; the difference between these lies in the different disposition of the hexagons, and the concomitant differences in chamber shape and stolon system may readily be appreciated.

Since the addition of new chambers was by secretion from drops or blebs of cytoplasm extruded from chambers already formed through the stoloniferous passages, it is logical to infer that the shape and pattern of arrangement of the chambers must have been linked with the pattern of the stolon system. The nature and extent of this linkage has not yet been worked out; but certain suppositions are permissible. For instance, in a pattern which allows of no common wall shared between adjacent chambers of the same growth annulus, it is scarcely probable that a connecting annular stolon will be found; this condition is illustrated in

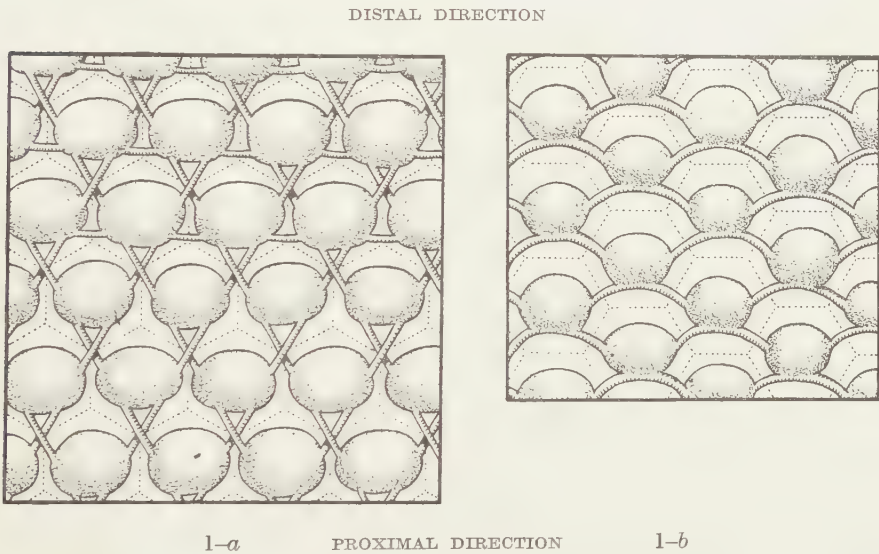


Fig. 1. Two types of equatorial chambers. Shown diagrammatically, as if based on regular hexagonal ground-plans differently oriented with respect to the proximal and distal direction. Correlation is apparent between the arrangement of the diagonal stolons and the chamber pattern. 1-a, Diagonal stolons crossed. Four stolon system in proximal rows, six stolon system in distal rows of chambers (Ex; *Lepidocyclus ocalana*, *L. supera*). 1-b, Diagonal stolons uncrossed. Four stolon system (Ex; *Lepidocyclus pustulosa*).

Figure 1b. In a pattern wherein adjacent chambers along the same radius are separated from one another by a long radial wall, no connecting radial stolon is likely; this condition is illustrated in Figure 1a.

It is further noticeable that in Figure 1a the diagonal stolons are crossed; in Figure 1b, they are uncrossed. Correlations between chamber shape and chamber pattern and stolon system can be attempted from diagrams, but what is required is not so much the hypothetical correlation as recording of the correlations actually to be seen in suitable preparations.

One further inference is logical; the position of the two diagonal stolons in relation to one another, and the angle at which they approach or cross one another, is likely to have affected or even determined the shape and pattern of the equatorial chambers.

All species of *Lepidocyclus* from which successful decalcified preparations have been made show the normal presence of four diagonal—or oblique—stolons to each equatorial chamber. Besides these there may be two annular stolons, connecting adjacent chambers of the same annulus, and two radial stolons. The presence of annular stolons seems to be normal when the two adjacent chambers share a common wall of appreciable length. The occurrence of radial stolons, on the contrary, seems to be for the most part irregular and fortuitous; in fact, in some cases in which radial stolons have been reported, the illustrations show what seem to be interpretable rather as stolons connecting lateral chambers only, or possibly passages from equatorial chamber to lateral chamber.

The diagonal stolons may lie in more than one plane of the equatorial layer, and in a number of species in which they occur in two distinct planes they may be clearly observed to cross one another. In a number of other species the diagonal stolons appear to lie in a single plane, but their relative positions are such that they cannot cross, though in some cases they approach one another and appear to be confluent. It will be seen that this distinction between crossed and uncrossed stolons seems to offer a means of separating, during the Upper Eocene at least, two distinct lineages within the genus *Lepidocyclus*, s.l., as here understood. This must be regarded as a fortunate circumstance, since this characteristic may be expected to undergo evolution and convergence like so many others.

By way of summarizing we may say that, in seeking to make use of the stolon system for taxonomic purposes it should be taken in conjunction with other characteristics of the equatorial chambers, with which it seems to be closely linked. The nature and extent of the linkage are matters of direct observation and recording from suitable preparations.

#### SPECIES AND SPECIES-GROUPS IN *LEPIDOCYCLINA*

A number of authors have remarked on the great variability, within the species, to be found among the *Lepidocyclus*es; few, however, seem to have been guided by their own precepts, since the majority are seen to have given way to the impulse—when confronted by a fresh population—to find in its distinctive aspect the grounds for erecting a new species or variety.

It is this „distinctive aspect“ of fresh populations which has so often tended to mislead even conservative taxonomists into forgetting, or at least discovering an excuse for not applying, their principles. It results—as is well known—from a shift in the mean of one or more characters



which obscures to some extent the similarity of the fresh population with those previously studied. If there be sufficient "overlap" of the range of characters displayed by diverse populations of a species, their conspecificity may be recognized and identity claimed, or perhaps an "affinity" may be indicated in the naming. But quite frequently this "overlap", especially between populations relatively remote from one another in space or stratigraphic interval, is insufficient for immediate apprehension of the relationship between them, in the absence of aid from intermediate populations, and a new species is proposed.

The reality of this tendency among palaeontologists—to overestimate the taxonomic significance of variable characteristics and so to erect superfluous species—is clear from various studies by VAUGHAN (1933), VAUGHAN and COLE (1941), and COLE (1944, 1952, 1953, 1957), wherein some 70 described species have become reduced by synonymization to about 15 accepted.

It is with this in mind that I have rigorously pruned the remaining "species" down to 23 species-groups, which suffice for most of the purposes of this article.

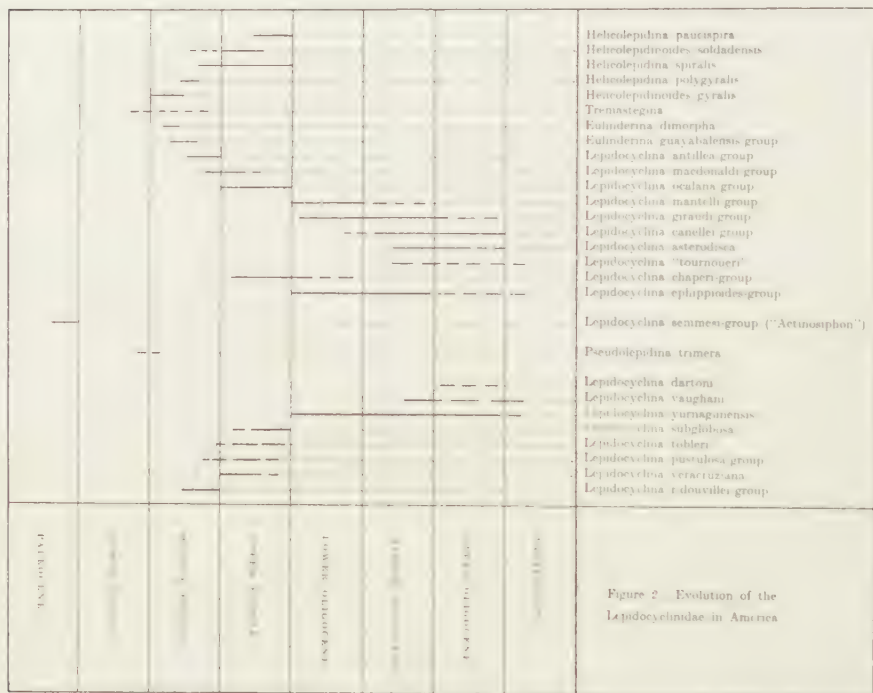
Here, then, is a bare list of the species-groups; their detailed contents will be found in Appendix 2, where important references to published synonymies are also provided.

1. Group of *Lepidocyclina antillea*
2.     "     "     "     *macdonaldi*
3.     "     "     "     *ocalana*
4.     "     "     "     *chaperi*
5.     "     "     "     *mantelli*
6.     "     "     "     *giraudi*
7.     "     "     "     *canellei*
- 7a.    "     "     "     *asterodisca*
8.     "     "     "     *ephippioides*
9.     "     "     "     *"tournoueri"*
10.    "     "     "     *r. douvillei*
11.    "     "     "     *veracruziana*
12.    "     "     "     *pustulosa*
13.    "     "     "     *tobleri*
14.    "     "     "     *yurnagunensis*
15.    "     "     "     *vaughani*
- 15a.   "     "     "     *dartoni*
16.    "     "     "     *semmesi*
17.    "     "     *Helicolepidina spiralis*
18.    "     "     "     *paucispira*
19.    "     "     *Helicolepidinoides gyralis*
20.    "     "     *Eulinderina guayabalensis*
21.    "     "     *Pseudolepidina trimera*

THE EVOLUTION OF THE *LEPIDOCYCLINIDAE*

We may now attempt a brief outline of the course of evolution in the two principal *Lepidocyclus* "Lineages"; the essence of this is expressed in Figure 2.

Genetic study of the *Lepidocyclus* resolves itself, in practice, into a comparative essay on the ground-plan of the equatorial layer which, as previously remarked, is found by experience so be the least unstable group of elements in the test of these plastic organisms, and in general the slowest to react to genetic change. All the characteristics of the test are subject to evolution. The nucleoconch has been shown (RENZ and



KÜPPER, 1947) to alter in time from "isolepidine" to "nephrolepidine" and, in one sublineage at least, to proceed to "eulepidine". Occasional aberrant stocks are thrown off with "pliolepidine" embryos, one of these has been regarded as a teratologic form, but I do not accept this explanation<sup>1)</sup>. They merit at least specific recognition, and possibly subgeneric.

The periembryonic chambers present changes from one to four spiral series opening from the primary auxiliary chamber or chambers in primitive species; later evolution comprises increase in the number of adauxiliary chambers, a trend observable in two or more sublineages.

<sup>1)</sup> It would be as sensible to claim all "nephrolepidine" forms as teratologic variants of "isolepidine", except that there are more of them and their traumas more varied and complex!



The main mass of equatorial chambers also shows progressive change in the pattern and shape of the chambers. The primitive arrangement seems to favour a radial design, the individual chambers being either approximately equidimensional (*L. peruviana*) or else longer tangentially than radially (*L. antillea*). The direction of evolution is towards a relative increase in the radial dimension, with corresponding decrease in the tangential, leading to the ultimate adoption of a radially elongate or spatulate oblong in a hexagonal framework; but in many species the variability of chamber shape renders a precise classification extremely difficult.

The concomitant change in stolon pattern is from a primitive condition of uncrossed diagonal stolons, with no annular stolons, to a crossing of the diagonal stolons and the appearance of annular stolons. This offers little basis for classification, were it not for the fact that the crossing of the stolons occurs sooner in one of the principal lineages than it does in the other. Thus throughout the Upper Eocene, and probably in part of the Oligocene also, the species of "Lineage Y" consistently show crossed diagonal stolons, whereas those of "Lineage X" are uncrossed. This presents us with a useful additional clue for the separation of the lineages, which is less liable to misinterpretation than the difficult guidance of chamber pattern and chamber shape.

#### A. EVOLUTION OF THE *LEPIDOCYCLINA* "LINEAGE Y"

*Eulinderina dimorpha* was derived from *Helicolepidinoides gyralis* through reduction of the primary spiral and its replacement outwards by chambers (or chamberlets) arising from the multiplication—through stoloniferous passages independent from the "Amphistegine" or interior-marginal aperture of the primary spiral chambers—of ventral arcuate chambers (or chamberlets) and their rapid spread beyond the primary spiral to form ultimately a complete ring, thus initiating radial-centrifugal growth from an annular growth-edge. (Where the extension of such arcuate chambers from the ventral side was confined within the limits of the primary spire, species of *Helicolepidina* arose. The connecting links between *Helicolepidinoides gyralis* and *Helicolepidina polygyralis* remain to be discovered, but it seems almost certain that *Helicolepidina paucispira* was a direct offshoot from *Helicolepidinoides soldadensis* (CRIMSDALE, in VAUGHAN and COLE, 1941, p. 86)).

Further reduction of the primary spiral, together with loss of the foraminal counter-septa, converted *Eulinderina semiradiata* into *Lepidocyclus antillea*, the most primitive form of which (*Lepidocyclus "barkeri"* of TAN, 1936) still shows only a single primary embryonic spiral. *L. antillea* first appears at about the middle of the Middle Eocene (Clai-borne: Guayabal of E. Mexico), evolving towards the end of the Middle Eocene into *L. proteiformis*, *L. discoidalis*, and *L. ariana*, the latter being the first representative of the *L. macdonaldi*-group.

*L. macdonaldi* probably transects the boundary Middle/Upper Eocene, where it gave rise to *L. montgomeriensis*, *L. gubernacula*, and the group of *L. ocalana*. *L. montgomeriensis* and *L. ocalana* are geologically the earliest species from which crossed diagonal stolons have been yet recorded, and both possess annular stolons at least in the distal parts of the equatorial layer (*L. ocalana*; see VAUGHAN and COLE, 1941, p. 116).

The nucleocoench of *L. ocalana*, though described as "isolepidine", frequently shows a tendency to become "nephrolepidine", passing thus into *L. "fragilis"*, recognized by Cole as a synonym of *L. chaperi*. The *L. chaperi*-group is found chiefly in the Upper Eocene, but a representative ranges into Oligocene (*L. leonensis*; see COLE, 1945, p. 34); the nucleocoench of species of the *L. chaperi*-group is quite consistently "nephrolepidine".

Both the *L. ocalana*- and the *L. chaperi*-groups possess descendants in the Oligocene. From *L. ocalana* were derived, on the one hand, *L. mantelli* and *L. supera* (like *L. ocalana*, both possess crossed diagonal stolons and *L. supera* has annular stolons in the distal equatorial chambers only), and on the other, the *L. giraudi* group. From the *L. chaperi*-group sprang the *L. "favosa"* = *L. ephippiodes*-group—the eulepidines of authors, all that have been studied possessing crossed diagonal stolons; the eulepidines persist into the Middle or Upper Oligocene of America, and into the Aquitanian of the Eastern Hemisphere.

From the *L. giraudi*-group, or possibly more directly from *L. supera* via *L. forresti* (if those two species are not synonymous), or from both arose the species of the *L. canellei*-group, with its stellate derivative *L. asterodisca*. *L. "californica"*, a synonym of *L. waylandcraighani* according to COLE (1957, p. 42), shows clear transitions of the more typical "isolepidine" nucleocoench to examples with "nephrolepidine" nucleocoench indistinguishable from the American species referred to *L. "tournoueri"*, and it is reasonable to infer, since all the other characters conform well enough, that *L. "tournoueri"* is descended from the *L. canellei* group.

All of these post-Eocene forms, with few exceptions, possess hexagonal-spatulate equatorial chambers in the main, which in some species become rather elongated in a radial direction. The lateral chambers are very variable, but show on the whole a tendency to more regular "tiered" arrangement than the majority of the Eocene species in the lineage.

#### B. EVOLUTION OF THE *LEPIDOCYCLINA* "LINEAGE X"

The earliest recognizable forms belonging to this lineage are species included in the *Lepidocyclus* *r. dourillei*-group. Populations identified as *L. peruviana* and *L. richayalcensis* are frequent in the Middle Eocene of Eastern Mexico, mainly south and southwest of the Poza Rica oilfield. *L. peruviana* is found in immediate and abundant association with *L. antillea*, and while the possibility of direct genetic connection between these two species cannot be dogmatically excluded, the striking differences between them renders in my opinion any close relationship unlikely.



Both equatorial ground-plan and the characters of the lateral chambers imply a far closer connection between *L. antillea* and *Eulinderina* than between either of these and the *L. r. douvillei*-group.

*L. peruviana* occurs also in the Middle Eocene <sup>1)</sup> of Florida, and "*Lepidocyclus* aff. *Lepidocyclus peruviana*—*r. douvillei* group" is reported from the Middle Eocene of Venezuela (VAN RAADSHOVEN, 1951, p. 482).

Whence did the *Lepidocyclus r. douvillei*-group spring, then? No satisfactory answer to this can yet be given, and for this reason I hesitate to separate "Lineage X" generically from "Lineage Y", retaining both provisionally in *Lepidocyclus* s.l. This is further discussed below in the chapter on Nomenclature of the lineages.

From the *L. r. douvillei*-group with, for the most part, sub-circular equatorial chambers having, in some variants of *L. peruviana* at least, strongly thickened distal walls in some portions of the layer, arose the *L. pustulosa*-group. The species of the latter group possess in the main ogival or diamond-shaped equatorial chambers, resembling those of *L. peruviana* in ground-plan, but relatively more elongate radially, while the thickening of the distal walls is proportionately less pronounced.

Two aberrant species of Upper Eocene age are probably derived one from each of the above. From *L. peruviana* sprang *L. veracruziana* in which the thickening of the walls of the equatorial chambers is heavily augmented to form an almost solid mass of shell material, infilling the central parts of the equatorial chambers and dividing the equatorial layer into three tiers. From *L. pustulosa* is thought to have descended *L. tobleri*, with its distinctive embryonic apparatus, but having the ground-plan of the equatorial chambers similar to that of *L. pustulosa* in other respects.

All the above species have uncrossed diagonal stolons, and none possess annular stolons, in the equatorial layer.

The *L. pustulosa*-group gave rise to *L. subglobosa* in the Upper Eocene, again with ogival to diamond-shaped equatorial chambers, but resembling the Oligocene *L. yurnagunensis* more closely than *L. pustulosa*, with more open lateral chambers than the latter. The widespread species *Lepidocyclus yurnagunensis* is the principal Oligocene representative of the "Lineage X", and it seems to have reached both the Mediterranean region — where it is known as *Lepidocyclus praetournoueri* H. DOUVILLÉ 1925 —

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<sup>1)</sup> The stratigraphic evidence from Florida and from Eastern Mexico for the Middle Eocene age of the *Lepidocyclus r. douvillei*-group is quite unequivocal. This implies that the Peruvian occurrences of *L. peruviana* and *L. vichayalensis* in association with *Helicolepidinoides soldadensis* in the Verdun formation (STAINFORTH, 1955, p. 2074, quoting CUSHMAN and STONE, 1949) must be due to large-scale reworking of Middle Eocene material, or else this part of the sequence belongs by right in the Middle Eocene and has been mistakenly correlated with the Upper Eocene. There is also local large scale reworking of Middle Eocene material in the base of the Upper Eocene Tantoyuca formation and its equivalents in Eastern Mexico; this is quite adequate to account for the presence of *Lepidocyclus proteiformis* and *L. veracruziana* in the same rock fragment, cited by COLE, 1956, p. 221.

and the Far East, — where it has been recorded as *Lepidocyclina isolepidinoides* VAN DER VLIERK and/or *L. parva* OPPENOORTH <sup>1)</sup>. In *L. yurnagunensis* it is possible to observe in the nucleoconch a complete series of transitions between “isolepidine” and “nephrolepidine” form, just as in *L. “californica”* of the “Lineage Y”.

“Nephrolepidine” forms of *L. yurnagunensis* grade into *L. vaughani* which, with the stellate *N. dartoni*, are believed to comprise the last forms of *Lepidocyclina* “Lineage X” in the Western Hemisphere.

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<sup>1)</sup> If specific identity is ever established, *L. parva* would appear to have priority. See OPPENOORTH, 1918.

(To be continued)

# EVOLUTION IN THE AMERICAN *LEPIDOCYCLINIDAE* (CAINOZOIC FORAMINIFERA): AN INTERIM REVIEW. II

BY

T. F. GRIMSDALE

(Communicated by Prof. I. M. VAN DER VLIERK at the meeting of Nov. 29, 1958)

## TAXONOMIC COMMENTS, AND THE NOMENCLATURE OF THE *LEPIDOCYCLINA* "LINEAGES"

The phylogenetic picture tentatively presented here and in Figure 2 cuts across the subgeneric division in general use today. The subgenera appear either as parallel stages of evolution in distinct lineages, or as aberrant offshoots from one or other of these. The first "nephrolepidines" develop as a branch of "Lineage Y" in the Upper Eocene (*Lepidocyclina chaperi*); while later, in Oligocene time, yet another "nephrolepidine" form is interpreted as having arisen from each major "Lineage", *Lepidocyclina vaughani* from "Lineage X", *L. "tournoueri"* from "Lineage Y". These three species are here regarded as the representatives of three supraspecific groups, many of which are to be found in the Eastern Hemisphere and are unmentioned here; conventionally, all are now assigned to the subgenus *Nephrolepidina*.

The "eulepidines" are believed to be an Oligocene development from the Upper Eocene "nephrolepidines". They persisted through the Aquitanian of the Eastern Hemisphere. A later stock resembling the "eulepidines", but apparently quite distinct from them, is the group of *Lepidocyclina rutenii*, which presumably evolved independently out of one of the two "nephrolepidine" stocks persisting from the late Oligocene.

The two or more "pliolepidine" species, *Lepidocyclina tobleri* in the Upper Eocene and *L. luxurians* in the Miocene, are considered to be aberrant end-forms, specifically distinct but scarcely meriting subgeneric recognition. This applies equally to *Lepidocyclina veracruziana*.

There can properly be a choice of but two names for *Lepidocyclina* "Lineage Y", since this embraces the species *Nummulites mantelli* MORTON 1833, the type species of *Lepidocyclina* GÜMBEL 1870 and also of *Cyclosiphon* EHRENBURG 1855. Both of these are nomenclatorially valid, *Cyclosiphon* having clear priority in time. Nevertheless, GALLOWAY's resurrection of this name in 1928 has been received by other palaeontologists—especially VAUGHAN (1929a, p. 29)—with disfavour, and it has been dropped from further use except by GALLOWAY himself (1933). Only an Opinion from



the Commission for Zoological Nomenclature can settle this matter beyond further dispute. For the purposes of the present account, *Lepidocyclina* has been preferred solely upon grounds of common usage.

*Lepidocyclina* "Lineage Y", then, has to remain *Lepidocyclina*, s.l.

For *Lepidocyclina* "Lineage X" it is quite impossible to select a name at this juncture with any confidence that it can be sustained. In the first place, the resemblance of its species to *Lepidocyclina* s.l. is so close, and its phylogenetic distinction therefrom so uncertain, that it would be premature to accord it a separate generic status.

Are subgeneric divisions warranted? For "Lineage X", the answer is 'Yes indeed'! But once more it is impossible to decide upon a subgeneric name until, for instance, the exact nature of *Lepidocyclina marginata* (MICHELOTTI) is established in terms of equatorial ground-plan, megalo-spheric nucleocoench, and stolon system. Then it may be possible to settle whether the name *Nephrolepidina* can be correctly applied to some part of *Lepidocyclina* "Lineage X" (*L. vaughani*), or of "Lineage Y" (*L. "tournoueri"*, perhaps), and how far the name may be extended to other groups of either lineage.

It is obligatory to use *Lepidocyclina* as a subgeneric name for the group of *L. mantelli*, but inadvisable to extend its use to other species-groups until more is known of the stolon patterns.

*Isorbitoina* is available for the *L. pustulosa*-group, should it be considered to merit subgeneric status; with *Pliolepidina* for *L. tobbleri*, and *Triplalepidina* for *L. veracruziana*.

Eocene species of "Lineage Y" having uncrossed stolons will fall reasonably into *Polylepidina* if they require subgeneric recognition, without regard to the variations of the nucleocoench and perieubryonic ring. *Eulepidina* is still valid for the species of the *L. ephippoides*-group.

Enough has been said to explain my decision to adopt a conservative attitude with regard to the generic and subgeneric nomenclature, employing only *Lepidocyclina* in its broadest sense and abandoning for the present all the subgeneric classification. It may seem to some undesirable ever to reinstate it; a multiplication of names of fossils is generally anathema to the stratigrapher.

#### A POSSIBLE CONTRIBUTORY CAUSE TO CONVERGENT EVOLUTION IN THE LARGE FORAMINIFERA

DROOGER (1956, pp. 464 et seq.) has discussed the acquisition of radial symmetry in the various families of large Foraminifera, offering his opinion that "... the increase in absolute size may be another adaptive response to the shallow water environment ...". My own speculations on this question were aroused long ago by a paper of CUSHMAN's (1930), in which he suggested that *Lepidocyclina*, like certain living large Foraminifera (representatives of the families *Pencroplidae*, *Alveolinidae*, and the genus

*Cycloclypeus*), might have adopted a commensal mode of life with unicellular green *Algae* — *Zooxanthellae*.

Commensalism of this kind has been studied in other groups of organisms, notably the reef corals. It is found that the basis of the relationship is, in effect, an exchange of waste products; the animal partner provides carbon dioxide from its respiration and nitrogenous matter from its nutritive metabolism, both of which the plant partner can use in its own life processes; the plant produces excess oxygen which serves to supplement the limited supply available in solution in the sea water, and also synthesizes carbohydrate which, in the case of the reef corals, is unassimilable by the animal which is exclusively carnivorous, but which for the omnivorous *Foraminifera* may serve as food. Thus both plant and animal partners benefit in two respects; (a) their respective waste products are consumed, and (b) their normal (external) sources of respiratory and nutritive supply are augmented from within.

The resulting physiological advantage to the Foraminifer would give impetus to its metabolism, increasing its potential speed of growth and reproduction. So long as the chamber addition is restricted to one at a time in the normal helical spiral of a rotalid genus, potential speed of growth is of small moment; but it places a premium upon mutations which provide extra apertures—mutations which would be of no significance unless the rate of metabolism could be increased. Thus the adoption of a radial symmetry in the large *Foraminifera* is a natural response to an augmented growth potential, which could be the result of colonization by *Algae*.

A commensal partnership in which a green plant plays a part becomes, as it were, secondarily phototropic, in that its life processes are favoured by strong light. This is a concomitant of shallow water, where the large *Foraminifera* of the present day mostly occur, and which is assumed to be the habitat of the fossil large *Foraminifera*.

This hypothesis offers a concrete reason for the convergent nature of evolution in the so-called "orbitoidal" *Foraminifera*, accounting for the remarkably similar range of form acquired, apparently independently, in a number of separate stocks.

It may be further speculated that the development of the lateral chambers is a mechanism for keeping apart the bulk of the *Algae* from the principal reproductive activities of the cytoplasm, assumed to have been carried on in the equatorial layer; the minimal importance of the lateral chambers for taxonomic purposes may be reflected in this explanation.

#### ACKNOWLEDGEMENTS

I am most happy to express my gratitude for the immense amount of help I have received, in the course of twenty-five years, from many specialists in the larger *Foraminifera* with whom I have been privileged

to discuss the *Lepidocyklinidae*. Foremost amongst these I must rate my former collaborator, Mr. R. WRIGHT BARKER, who assisted in the birth of much of the hypothesis here outlined. Others, most notably, are Miss CAUDRI, Prof. VAN DER VLERK, and the late Dr. TAN SIN HOK. But I feel almost equally indebted to many others from whose published works have been culled by far the most of the essential facts upon which this review is grounded. The list of references will provide a rough estimate of their contributions, and is in itself a form of acknowledgement.

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#### APPENDIX I

#### GENERIC ANALYSIS OF THE *LEPIDOCYKLINIDAE*

In the following list, generic or subgeneric names which are nomenclatorially valid are shown in capitals. Those marked with an asterisk are for the time being placed in disuse, even though they may be valid nomenclatorially.

- \* *ACTINOSIPHON* VAUGHAN, 1929-b, p. 163. Type species, *Actinosiphon semmesi* VAUGHAN, 1929-b, p. 164, pl. 21, figs. 1-3.

It is considered that this species possesses no feature by which it can be satisfactorily differentiated from *Lepidocyclus*, s.l.

- \* *AMPHILEPIDINA* H. DOUVILLÉ, 1922. Subgenus of *Lepidocyclus*. Type species, designated by H. DOUVILLÉ (1925, p. 100), *Lepidocyclus sumatrensis* (H. B. Brady) = *Orbitoides sumatrensis* H. B. BRADY, 1875.

*Astrolepidina* SILVESTRI, 1931, p. 35. Subgenus of *Lepidocyclus*. Type species, not cited.

Proposed by SILVESTRI for the stellate forms of *Lepidocyclus*, it was rejected by VAUGHAN (1933-b, p. 923) on the grounds that SILVESTRI "named no type-species of the subgenus and distinguished it . . . solely by its stellate form".

- \* *CYCLOLEPIDINA* WHIPPLE, 1934, p. 143. Subgenus of *Lepidocyclus*. Type species, *Lepidocyclus (Cyclolepidina) suvaensis* WHIPPLE, 1934, p. 145, pl. 20, fig. 1-8; text-fig. 11-b = *Lepidocyclus (Pliolepidina?) luxurians* TOBLER, 1925, p. 269, pl. 8, fig. 1-4 (teste CAUDRI, 1939, p. 232).

*Cyclolepidina* is a synonym of *Multilepidina* HANZAWA 1932 (teste VAUGHAN and COLE, in CUSHMAN, 1940) and must therefore lapse.

*Cyclosiphon* EHRENBERG, 1855, teste GALLOWAY, 1928. Type species, by monotypy, *Cyclosiphon mantelli* (MORTON) = *Nummulites mantelli* MORTON, 1833, p. 291, pl. 5, fig. 9.

Rejected by VAUGHAN (1929, pp. 28, 29) on the grounds of EHRENBERG's unreliable characterization; this rejection has been sustained by all subsequent authors except GALLOWAY, 1933, pp. 433, 434.

Whilst the grounds for this rejection are dubious, since a described and figured species was cited by the author as belonging to his genus, the name *Lepidocyclus*—a typonym of *Cyclosiphon* according to GALLOWAY—has been established by some sixty years of usage prior to GALLOWAY's revival of *Cyclosiphon*. *Lepidocyclus* GÜMBEL, 1868 is therefore retained in preference.



- \* *EOLEPIDINA* TAN SIN HOK, 1939, p. 69. Type species, designated by TAN, 1939, *Eulinderina semiradiata* BARKER and GRIMSDALE, 1936, p. 238, pl. 30, figs. 10-12; pl. 32, figs. 15-17; pl. 35, figs. 1-3; pl. 37, fig. 1; emend. TAN SIN HOK, 1936, p. 246.

Rejected here as a synonym of *Eulinderina* BARKER and GRIMSDALE, or of *Lepidocyclus* GÜMBEL.

- \* *EULEPIDINA* H. DOUVILLÉ, 1911, p. 59. Subgenus of *Lepidocyclus*. Type species, designated by YABE, 1919, p. 41. *Lepidocyclus (Eulepidina) dilatata* (MICHELOTTI) = *Orbitoides dilatata* MICHELOTTI, 1861, p. 17, pl. 1, figs. 1, 2.

This name, together with several other subgeneric names, is omitted from use in the present study, though it may be desirable subsequently to reintroduce it.

- EULINDERINA* BARKER and GRIMSDALE, 1936. Type species, designated by BARKER and GRIMSDALE, 1936, *Eulinderina guayabalensis* (NUTTALL) = *Planorbulina (Planorbulinella) guayabalensis* NUTTALL, 1930.

The name is retained for the present as a distinct genus, though it is debatable whether *Lepidocyclus* deserves to be emended so as to include it.

- \* *HELICOCYCLINA* TAN SIN HOK, 1936, p. 254. Type species, designated by TAN, 1936, *Helicocyclus paucispira* (BARKER and GRIMSDALE) = *Helicolepidina paucispira* BARKER and GRIMSDALE, 1936, p. 243, pl. 31, figs. 11, 12; pl. 33, figs. 4-6; pl. 36, figs. 1, 3; pl. 38, fig. 4.

It is here taken to be a synonym of *Helicolepidina*; but the name is nomenclatorially valid and it may be desirable subsequently to reintroduce it.

- HELICOLEPIDINA* TOBLER, 1922, p. 380. Proposed as a subgenus of *Lepidocyclus*, it was raised to generic rank by GALLOWAY, 1928, p. 60. Type species, by monotypy, *Helicolepidina spiralis* (TOBLER) = *Lepidocyclus (Helicolepidina) spiralis* TOBLER, 1922, p. 380, text-figs. 1-3.

- HELICOLEPIDINOIDES* TAN SIN HOK, 1936, p. 254. Type species, designated by TAN, 1936, *Helicolepidinoides gyralis* (BARKER and GRIMSDALE) = *Helicostegina gyralis* BARKER and GRIMSDALE, 1936, p. 236, pl. 30, figs. 3-5; pl. 32, figs. 4, 5; pl. 34, figs. 2-6; pl. 37, fig. 6; text-fig. 2.

- \* *HELICOSTEGINA* BARKER and GRIMSDALE, 1936, p. 233. Type species, designated by BARKER and GRIMSDALE, *Helicostegina dimorpha* BARKER and GRIMSDALE, 1936, p. 235, pl. 30, fig. 6; pl. 32, figs. 6, 7; pl. 33, fig. 9; pl. 34, figs. 7, 9; pl. 37, fig. 3.

TAN (1936, p. 243 et seq.), refers the species *dimorpha* to the genus *Eulinderina*; this leaves *Helicostegina* without a type species, rendering it invalid.

- Isolepidina* H. DOUVILLÉ, 1915-b, p. 724. Subgenus of *Lepidocyclus*. Type species, *Lepidocyclus (Isolepidina) mantelli* (MORTON) = *Nummulites mantelli* MORTON 1833.

This is a typonym of *Lepidocyclus* GÜMBEL and of *Cyclosiphon* EHRENBERG.

- \* *ISORBITOINA* THALMANN, 1938, p. 202. Type species, designated by THALMANN, 1938, *Lepidocyclus trinitatis* H. DOUVILLÉ, 1924. According to VAUGHAN and COLE, 1941, p. 65, *L. trinitatis* is a synonym of *L. pustulosa* H. DOUVILLÉ, 1917, and this view is accepted.

- LEPIDOCYCLINA* GÜMBEL, 1868 (1870), pp. 689, 717. Type species (designated by H. DOUVILLÉ, 1898, p. 594), *Lepidocyclus mantelli* (MORTON) = *Nummulites mantelli* MORTON, 1833.

- Multicyclus* CUSHMAN, 1919, p. 96. Subgenus of *Lepidocyclus*. Type species, by monotypy, *Lepidocyclus (Multicyclus) duplicata* GUSHMAN 1919 = *Lepidocyclus (Pliolepidina) tobleri* H. DOUVILLÉ, 1917, p. 844, text-figs. 5, 6 (teste VAUGHAN, 1933-c, p. 295).

Vaughan's recognition of *Lepidocyclus duplicata* as a synonym of *L. tobleri* renders *Multicyclus* a typonym of *Pliolepidina* H. DOUVILLÉ, 1915.

- \* *MULTILEPIDINA* HANZAWA, 1932, p. 447. Subgenus of *Lepidocyclus*. Type species, designated by HANZAWA, 1932, *Lepidocyclus (Multilepidina) irregularis* HANZAWA, 1932, p. 448, figs. 1-6.

The name, together with several other subgeneric names, is omitted from use in the present study, though it may be desirable subsequently to reintroduce it.

- Neolepidina* BRÖNNIMANN, 1946, p. 378. Subgenus of *Lepidocyclus*. Type species, designated by BRÖNNIMANN, 1946, *Lepidocyclus (Neolepidina) pustulosa* H. DOUVILLÉ = *Lepidocyclus (Isolepidina) pustulosa* H. DOUVILLÉ, 1917, p. 844, text-figs. 1-4.

*Neolepidina* is thus a typonym of *Isorbitoia* THALMANN 1938, since BRÖNNIMANN himself accepts, as I do, the contention of VAUGHAN and COLE (1941) that *L. trinitatis* H. DOUVILLÉ is a synonym of *L. pustulosa* H. DOUVILLÉ.

- \* *NEPHROLEPIDINA* H. DOUVILLÉ, 1911, pp. 59, 73. Subgenus of *Lepidocyclus*. Type species, designated by YABE, 1919, p. 41. *Lepidocyclus marginata* (MICHELOTTI) = *Nummulites marginata* MICHELOTTI, 1841.

*Orbitoia* VAN DEN GEYN and VAN DER VLIERK, 1935, p. 227. No type species has been designated, therefore the name is nomenclatorially invalid.

- \* *PLIOLEPIDINA* H. DOUVILLÉ, 1915-b, p. 727, fig. 34. Subgenus of *Lepidocyclus*. Type species, an unnamed form in DOUVILLÉ's 1915 paper, which was afterwards named, by DOUVILLÉ himself, *Lepidocyclus (Pliolepidina) tobleri* H. DOUVILLÉ, 1917. See discussion by GRIMSDALE and VAN DER VLIERK, 1958, in press.

*Pliorbitoia* VAN DE GEYN and VAN DER VLIERK, 1935, p. 227. Subgenus of *Orbitoia* VAN DE GEYN and VAN DER VLIERK. Type species, by monotypy, *Orbitoia (Pliorbitoia) tobleri* (H. DOUVILLÉ) = *Lepidocyclus (Pliolepidina) tobleri* H. DOUVILLÉ, 1917, p. 844, text-figs. 5, 6.

The name is a typonym of *Pliolepidina* H. DOUVILLÉ, 1915, and must therefore be allowed to lapse.

- \* *POLYLEPIDINA* VAUGHAN, 1924, p. 807. Subgenus of *Lepidocyclus*. Type species, designated by VAUGHAN, 1924, *Lepidocyclus (Polylepidina) chiapasensis* VAUGHAN, 1924, p. 808, pl. 30, figs. 1-3; text-fig. 4 = *Lepidocyclus antillea* CUSHMAN, 1919 (teste COLE, 1944, p. 58 et seq.).

Raised to generic rank by GALLOWAY, 1928, p. 60. This status was sustained by BARKER and GRIMSDALE, 1936, p. 239, but rejected by COLE (various works) and by VAUGHAN and COLE, 1940, in CUSHMAN.

The name is valid nomenclatorially but, together with several other subgenera, is omitted from use in the present study, though it may be desirable subsequently to reinstate it.

- \* *POLYORBITOINA* VAN DE GEYN and VAN DER VLIERK, 1935, p. 227. Proposed as a subgenus of *Orbitoia* VAN DE GEYN and VAN DER VLIERK, 1935 q.v. Type species, by monotypy, *Orbitoia (Polyorbitoia) proteiformis* (VAUGHAN) = *Lepidocyclus (Polylepidina) proteiformis* VAUGHAN, 1924, p. 810, pl. 32, figs. 1-7; text-fig. 6.

The name is nomenclatorially valid but, together with several other subgeneric names, is omitted from use in the present study, though it may be desirable subsequently to reintroduce it.

*PSEUDOLEPIDINA* BARKER and GRIMSDALE, 1937, p. 169. Type species, designated by the authors, *Pseudolepidina trimera* BARKER and GRIMSDALE, 1937, p. 171, pl. 5, figs. 1-3; pl. 8, figs. 1-5; text-figs. 1, 2.

- \* *TRIPLALEPIDINA* VAUGHAN and COLE, 1938, p. 167. Type species designated by the authors, *Triplalepidina veracruziana* VAUGHAN and COLE, 1938, p. 167, pl. 27, figs. 1-9.

- \* *TRYBLIOLEPIDINA* VAN DER VLERK, 1928, p. 13. Type species, designated by WILLARD BERRY, 1929, p. 37, *Lepidocyclus* (*Trybliolepidina*) *ephippioides* (JONES and CHAPMAN) = *Orbitoides* (*Lepidocyclus*) *ephippioides* JONES and CHAPMAN, 1900.

GRIMSDALE and VAN DER VLERK (1958, in press) regard *L. ephippoides* as referable to the subgenus *Eulepidina*, rendering *Trybliolepidina* a (subjective) synonym of *Eulepidina*.

## APPENDIX 2

### SPECIES-GROUPS IN THE AMERICAN *LEPIDOCYCLINIDAE*

Since it is deemed impracticable to present species descriptions and full synonymies with this essentially preliminary account, the principal recognizable species of *Lepidocyclus* and allied genera are here disposed in a limited number of species-groups. In the case of species for which important lists of synonyms have already been published, the source of these is mentioned and the degree of my conformity expressed. It is too soon to decide whether these species-groups are to be looked upon as constituting subgenera or lower taxonomic units; in some cases it is likely that they represent single species to which more than one name has been applied, and in several cases they have actually been described as subgenera or even as genera<sup>1)</sup>, which are for the moment discarded.

The species-groups fall into four categories:

- A. Species-groups assigned to *Lepidocyclus* "Lineage Y".
- B. Species-groups assigned to *Lepidocyclus* "Lineage X".
- C.i. Species-groups here assigned to *Lepidocyclus* but not to either lineage.
- C.ii. Species-groups of other genera.

Acceptance of the synonymies cited is, of course, with reservations, and later modifications will undoubtedly be necessary. The same holds for the assignment of species to groups, since it is not always possible to be sure at present where every species should have its rightful place. To take an example: *Lepidocyclus forresti* VAUGHAN has lately been placed by COLE (1953, p. 334) in the synonymy of *Lepidocyclus supera* (Conrad). Their relationship is apparent from the type figure of *L. forresti*, but their identity—in my opinion—not so. Moreover interpretations of *L. forresti* by other authors (e.g., GRAVELL, 1933, pl. 3, fig. 6; VAUGHAN and COLE, 1941, pl. 35, fig. 3) suggest rather that it is related to *Lepidocyclus waylandvaughani* COLE, which would lead one to place it in the group of *L. canellei* instead of in that of *L. supera*.

#### A. Species-groups assigned to *Lepidocyclus* "Lineage Y"

##### 1. Group of *Lepidocyclus antillea*

*antillea* CUSHMAN, 1919 (Synonymy by COLE, 1944, pp. 57, 58 accepted).  
*discoidalis* (BARKER and GRIMSDALE), 1936.  
*proteiformis* VAUGHAN, 1924.

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<sup>1)</sup> Thus, Group 1 = *Polylepidina* VAUGHAN; Group 8 = *Eulepidina* H. DOUVILLÉ; Group 11 = *Triplalepidina* VAUGHAN and COLE; Group 13 = *Pliolepidina* H. DOUVILLÉ; Group 16 = *Actinosiphon* VAUGHAN; Group 17 = *Helicolepidina* TOBLER; Group 18 = *Helicocyclus* TAN; Groups 19, 20 and 21 are also generically distinct.



2. Group of *Lepidocyclina macdonaldi*  
*ariana* COLE and PONTON, 1934 (Synonymy by COLE, 1944, p. 61, accepted).  
*gubernacula* COLE, 1952.  
*macdonaldi* CUSHMAN, 1919.  
*meinzeri* VAUGHAN, 1933.  
*montgomeriensis* COLE, 1949.
3. Group of *Lepidocyclina ocalana*  
*novitasensis* VAUGHAN, 1933.  
*ocalana* CUSHMAN, 1920 (All the varieties previously recognized, as by VAUGHAN, 1928, p. 156, have been combined in the species by COLE, 1957, p. 32; this is accepted.).  
*tschoppi* THIADENS, 1937.
4. Group of *Lepidocyclina chaperi*  
*chaperi* LEMOINE and R. DOUVILLÉ, 1904 (Synonymy of COLE, 1952, pp. 23, 24 accepted, but without *L. tschoppi* THIADENS).  
*georgiana* CUSHMAN, 1920 (Accepting the view of VAUGHAN, 1924, p. 799 rather than COLE's, 1952, pp. 23, 25).  
*leonensis* COLE, 1945.  
*? persimilis* H. DOUVILLÉ, 1924.  
*? piedrasensis* VAUGHAN, 1933.  
*suwanneensis* COLE, 1945.
5. Group of *Lepidocyclina mantelli*  
*? forresti* VAUGHAN, 1927 (Following the view of COLE, that *L. forresti* is related to *L. supera* (COLE, 1953, p. 334) rather than to *L. waylandvaughani* COLE).  
*? hilli* CUSHMAN, 1920.  
*mantelli* (MORTON), 1933.  
*mantelli* var. *papillata* VAUGHAN, 1927.  
*supera* (CONRAD), 1866. (In a recent work COLE, 1957, p. 38 has placed *L. supera* in the synonymy of *L. mantelli*. The two are closely related, but their stratigraphic distribution seems to warrant their distinction).
6. Group of *Lepidocyclina giraudi*  
*giraudi* R. DOUVILLÉ, 1907 (Synonymy of COLE, 1957, p. 41 accepted).  
*? hodgensis* VAUGHAN and COLE, 1933.
7. Group of *Lepidocyclina canellei*  
*canellei* LEMOINE and R. DOUVILLÉ, 1904 (Synonymy of COLE, 1957, p. 33 accepted).  
*colei* GRAVELL and HANNA, 1937.  
*miraflorensis* VAUGHAN, 1923.  
*? sanluisensis* GRAVELL, 1933.  
*waylandvaughani* COLE, 1928 (Synonymy of COLE, 1957, p. 42, accepted).
- 7-a. Group of *Lepidocyclina asterodisca*  
*asterodisca* NUTTALL, 1932 (Synonymy of COLE, 1952, p. 17 accepted).  
*wetherellensis* VAUGHAN and COLE, 1933.
8. Group of *Lepidocyclina ephippioides*  
*ephippioides* (JONES and CHAPMAN), 1900 (For synonymy see GRIMSDALE 1952, pp. 240-242, and (for *L. favosa*) VAUGHAN, 1933, p. 37).

- gigas* CUSHMAN, 1919.  
*gigas* var. *duncanensis* COLE, 1934.  
*gigas* var. *mexicana* CUSHMAN, 1920.  
*petri* THIADENS, 1937.  
*undosa* CUSHMAN, 1919 (Synonymy of VAN DE GEYN and VAN DER VLIERK, 1935, p. 249, accepted).  
*undosa* var. *tumida* VAUGHAN, 1926.

9. Group of *Lepidocyclina* "*tournoueri*"  
*tournoueri* LEMOINE and R. DOUVILLÉ, 1904, teste VAUGHAN 1924. (Synonymy of COLE, 1952, p. 28 accepted)<sup>1</sup>.

B. *Species-groups assigned to Lepidocyclina "Lineage X"*

10. Group of *Lepidocyclina* *r. douvillei*  
*? atascaderensis* BERRY, 1930 (Synonymy of TODD, 1933, p. 348 accepted).  
*cedarkeysensis* COLE, 1942.  
*ecuadorensis* HOFKER, 1956.  
*? kugleri* GORTER and VAN DER VLIERK, 1932.  
*peruviana* CUSHMAN, 1922.  
*r. douvillei* LISSON, 1921.  
*r. douvillei* var. *armata* L. RUTTEN, 1928.  
*vichayalensis* L. RUTTEN, 1928.

Also

- Species A VAN RAADSHOVEN, 1951.  
 Species B VAN RAADSHOVEN, 1951.

11. Group of *Lepidocyclina* *veracruziana*  
*veracruziana* (VAUGHAN and COLE), 1938.
12. Group of *Lepidocyclina* *pustulosa*  
*? aurarensis* HODSON, 1926.  
*? hieronymi* M. G. RUTTEN and L. VERMUNT, 1932.  
*? maracaibensis* HODSON, 1926.  
*pustulosa* H. DOUVILLÉ, 1917 (Synonymy of VAUGHAN and COLE, 1941, p. 65, accepted for the most part; but not *Lepidocyclina kugleri* GORTER and VAN DER VLIERK, 1932, which probably belongs in the group of *Lepidocyclina r. douvillei*).  
*? schotborghi* M. G. RUTTEN and L. VERMUNT, 1932.  
*? sherwoodensis* VAUGHAN, 1928.  
*? variabilis* L. RUTTEN, 1928.

13. Group of *Lepidocyclina* *tobleri*  
*tobleri* H. DOUVILLÉ, 1917 (Synonymies of VAUGHAN and COLE, 1941, p. 66, and of COLE, 1952, p. 17, accepted).

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<sup>1</sup>) LEMOINE and R. DOUVILLÉ expressly designated the vertical section (1904, pl. 1, fig. 5) as the holotype of their species *Lepidocyclina tournoueri* (see note on their "contents" page, following p. 41). Since the equatorial sections assigned to *L. tournoueri* in Europe are of two distinct types—possibly distinct species at least, namely those with hexagonal-spatulate equatorial chambers and those with diamond-shaped chambers—it is necessary to await restudy of topotype material in equatorial section before *L. tournoueri* can be properly determined.

14. Group of *Lepidocyclina yurnagunensis*  
*yurnagunensis* CUSHMAN, 1919. (Synonym, *L. yurnagunensis* var. *morganopsis* VAUGHAN, 1933).  
*subglobosa* NUTTALL, 1928<sup>1</sup>).
15. Group of *Lepidocyclina vaughani*  
*vaughani* CUSHMAN, 1919 (Synonymy of COLE, 1952, pp. 29, 30, accepted).  
*? crassimargo* VAUGHAN, 1933.
- 15-a. Group of *Lepidocyclina dartoni*  
*dartoni* VAUGHAN, 1933.

C.i. *Species-groups here assigned to Lepidocyclina but not to either lineage*

16. Group of *Lepidocyclina semmesi*  
*semmesi* (VAUGHAN), 1929 (= *Actinosiphon semmesi*, not *L. (Nephrolepidina)* *semmesi* VAUGHAN and COLE, 1933).  
*berbadensis* VAUGHAN, 1945.  
*barbadensis* var. *caudriae* DE CIZANCOURT, 1951.  
*barbadensis* var. *garapatensis* DE CIZANCOURT, 1951.  
*barbadensis* var. *globulosa* DE CIZANCOURT, 1951.

C.ii. *Species-groups of other genera*

17. Group of *Helicolepidina spiralis*  
*august-tobleri* W. BERRY, 1932.  
*polygyralis* BARKER, 1932.  
*spiralis* (TOBLER), 1922 (Synonymy of COLE, 1952, p. 30, and of VAUGHAN and COLE, 1941, p. 76, accepted. See also VAN RAADSHOVEN, 1951, pp. 485-487).  
*spiralis* var. *trinitatensis* BRÖNNIMANN, 1944.
18. Group of *Helicolepidina paucispira*  
*paucispira* BARKER and GRIMSDALE, 1936.
19. Group of *Helicolepidinoides gyralis*  
*gyralis* (BARKER and GRIMSDALE), 1936.  
*soldadensis* (GRIMSDALE), 1941.
20. Group of *Eulinderina guayabalensis*  
*dimorpha* (BARKER and GRIMSDALE), 1936.  
*guayabalensis* (NUTTALL), 1930.  
*guayabalensis* var. *regularis* BARKER and GRIMSDALE, 1936.  
*semiradiata* BARKER and GRIMSDALE, 1936.
21. Group of *Pseudolepidina trimera*  
*trimera* BARKER and GRIMSDALE, 1937.

<sup>1</sup>) COLE (1952, p. 16) places *Lepidocyclina subglobosa* NUTTALL in the synonymy of *L. pustulosa* H. DOUVILLÉ. I found no difficulty in separating these two species during my work in Trinidad, but on the contrary I had considerable trouble separating *L. subglobosa* from *L. yurnagunensis*. From this I infer that *L. subglobosa* is closely related to both the other species and bridges the stratigraphic interval between them.



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# ABSORPTION DE L'HYDROGÈNE PAR DES MONOCRISTAUX DE PALLADIUM

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## Résumé

La plupart des recherches très nombreuses sur l'absorption de l'hydrogène par le palladium, soit par voie électrolytique soit en sortant de la phase gazeuse, ont été exécutées avec du palladium polycristallin. Dans une recherche récente Messieurs SUGENO et KAWABE [4] ont utilisé comme cathode des "monocristaux" mesurants quelques millimètres, ce qui permet aux auteurs d'étudier la déformation du réseau cristallin.

Des recherches pas encore finies, utilisant des monocristaux avec un diamètre de 0,5 jusqu'à 5 mm et une longueur de plusieurs centimètres, montrent que le réseau du monocristal après transformation dans la phase  $\beta$  ( $\text{Pd H}_{0,6}$ ) est changé dans un cristal "quasi-mosaïque" avec une dispersion des orientations de quelques degrés. Cette structure se conserve après dégagement de l'hydrogène.

Des microphotographies d'une face taillée d'un monocristal parallèle à  $\{100\}$  montrent, dans l'état  $\beta$ , des lignes de fracture ("rifts") ou lignes de glissement parallèles aux intersections des plans  $\{111\}$  avec la face taillée, en accord avec les conclusions de Sugeno et Kawabe.

## 1. Introduction.

L'absorption de l'hydrogène par le palladium a été le sujet de nombreuses recherches. Le système est remarquable parce qu'il montre deux phases lesquelles se distinguent seulement par la dimension du réseau cristallin. Ainsi que le palladium pur, ces deux phases ont une structure cubique faces-centrées. La dissolution de l'hydrogène produit d'abord, pour des petites quantités jusqu'à une composition d'environ  $\text{PdH}_{0,03}$  une légère dilatation du réseau: phase  $\alpha$  (le paramètre accroît de 3.883 Å à environ 3.894 Å). Puis il se forme la phase  $\beta$  avec une composition d'environ  $\text{PdH}_{0,6}$  à la température ordinaire et un paramètre du réseau beaucoup plus grand (4.017 Å). Les compositions des phases limites  $\alpha$  et

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$\beta$  dépendent de la température et, d'après SMITH [1], il n'y a plus deux phases au dessus d'une température "critique" d'environ  $310^{\circ}\text{C}$ , mais une seule phase dont le paramètre du réseau augmente continuellement avec une quantité croissante d'hydrogène.

L'hydrogène peut être absorbé de la phase gazeuse, ou bien par voie électrolytique. La vitesse de l'absorption (et de la désorption) dépend considérablement de l'état de la surface du métal, et dans cette connection, du milieu ambiant, comme par exemple un gaz, un acide dilué et encore pour la désorption, un liquide comme l'alcool. En outre des recherches exécutées avec des préparations ayant des dimensions différentes, par exemple de la poudre, des éprouvettes solides minces (quelques dixièmes d'un mm) ou plus épaisses (un demi centimètre) peuvent fournir des résultats bien différents quant aux phases présentes. C'est dû au fait que la diffraction des rayons-X, à cause de leur absorption élevée dans le palladium, ne présente que des données concernant la couche superficielle. Donc, en particulier pour les préparations épaisses, on n'est jamais sûr que la distribution des phases est la même dans toute l'extension du spécimen. Il est bien possible que les résultats de recherches électrochimiques (mesure du potentiel électrochimique dans divers milieux: voir [8]) lesquels présentent des difficultés quant à leur interprétation en termes de structure, sont influencés par de telles complications.

## 2. *Changement structurel accompagnant l'absorption de l'hydrogène*

La plupart des recherches ont été exécutées avec des préparations polycristallines. La présente recherche s'occupe de monocristaux.

Nous avons repris la recherche du système dans le but d'obtenir des données plus précises sur le changement structurel qui accompagne l'absorption de l'hydrogène par le métal.

On sait des recherches de plusieurs auteurs que la formation de la phase  $\beta$  cause une déformation du réseau, ce qui se révèle du fait que les lignes de diffraction dans un diagramme de poudre, à part de leur déplacement par suite de l'extension du réseau, s'élargissent. Ça veut dire qu'ils existent des petites différences dans les dimensions du réseau dans les divers domaines cristallins, ou bien que ces domaines soient extrêmement petits.

D'un tel diagramme de poudre on ne peut pas déduire si les cristallites individuels diffèrent en paramètre ou si un seul individu cristallin présente de telles différences. Une incertitude analogue se présente quant aux diagrammes qui montrent les lignes, et donc la présence, des deux phases  $\alpha$  et  $\beta$  simultanément. La aussi, on peut se demander si cet état comporte des cristallites  $\alpha$  à part de cristallites  $\beta$ , ou si un seul cristallite doit être considérée comme partiellement  $\alpha$  et partiellement  $\beta$ . De telles difficultés pourraient être résolues par l'étude de préparations monocristallines.

Un autre phénomène accompagnant la formation de la phase  $\beta$  est

l'apparition de certaines irrégularités à la surface du métal. La déformation du réseau cristallin par suite de la formation de la phase  $\beta$ , laquelle donne naissance à une augmentation du volume du métal, fut étudiée par plusieurs auteurs par voie micrographique. De ces travaux nous citons spécialement celui de SMITH et DERGE [2]. Ces auteurs ont observé que les cristallites individuels montrent des régions gonflées suivant des directions à peu près parallèles et de plus l'apparition de "lignes" bien définies. Dans la supposition que dans la texture du métal examiné les cristallites ont un plan  $\{110\}$  parallèle au plan de laminage et une direction  $\langle 100 \rangle$  parallèle à la direction de laminage, les auteurs concluent que les stries gonflées s'étendent parallèles à l'intersection de plans octaédriques du réseau. Une telle conclusion doit être considérée avec prudence, dû au fait que les orientations des cristallites individuels dans une texture laminée montrent toutefois une dispersion considérable, ce qui le fait incertain si un cristallite choisi occupe vraiment la position moyenne (déduite par TAMMANN [3] par voie optique).

Cependant les résultats montrent à tout cas que les stries gonflées suivent des directions bien définies.

Dans un travail plus récent, SUGENO et KAWABE [4] ont préparé par recristallisation, des éprouvettes de palladium avec des cristallites plus grand (avec des dimensions de quelques millimètres), ce qui leur a permis de faire des photographies suivant Laue de cristallites individuels. Ces photographies montrent, par leurs taches élongées (phénomène d'astérisme) bien claire que dans l'état  $\beta$  les cristallites individuels sont considérablement déformés. En outre ces auteurs ont pu constater la présence de lignes parallèles dans cette phase. Selon leurs données les lignes suivent également les plans  $\{111\}$  du réseau. Bien qu'ils ne le disent pas explicitement, ce résultat fut probablement obtenu en déterminant l'orientation des cristaux individuels au moyen de photographies Laue.

### 3. Préparation des monocristaux

Sans être au courant de ces dernières recherches, nous avons préparé des monocristaux de palladium de diverses dimensions. La préparation des cristaux fut exécutée dans le vide ou dans une atmosphère d'argon, soit en utilisant un processus de zone fondue en faisant passer une tube d'alumine par un anneau de graphite chauffé par haute fréquence, soit en faisant passer un creuset cylindrique par un four. Les cristaux obtenus de cette manière avaient une forme cylindrique avec un diamètre variant de 0,5 mm jusqu'à 5 mm et une longueur de 10 cm maximum. Les cristaux d'un diamètre de 5 mm nous ont permis, après avoir déterminé l'orientation du réseau à l'aide d'une photographie de Laue, d'en couper des plans bien définis, à savoir un plan (100) et un plan (111).

Les cristaux furent chargés avec l'hydrogène par voie électrolytique en utilisant une solution de  $\text{H}_2\text{SO}_4$  diluée (titre 0,1 N) et une densité de courant variant d'une dizaine jusqu'à quelques centaines de mA/cm<sup>2</sup>.



Dans ce qui suit nous montrons quelques résultats obtenus jusqu'à présent. Ils ne présentent que des résultats préliminaires, lesquels nous espérons compléter dans la continuation de cette recherche.

#### 4. Résultats

La figure 1 montre une série de photographies-X, obtenues avec un cristal cylindrique (diamètre 5 mm), le spécimen étant tourné autour de son axe. Le montage du film était tel, que les réflexions de haute ordre (sensitives pour des petites variations dans le paramètre du réseau atomique) se trouvent au centre du film (montage selon VAN ARKEL [5]).

La figure 1a provient du cristal non chargé. La figure 1b provient du même cristal après être chargé durant 2 heures avec une densité de courant de 280 mA/cm<sup>2</sup>. L'augmentation dans la distance des réflexions correspond à la formation du réseau  $\beta$ . La comparaison avec fig. 1a montre clairement que le réseau monocristallin est resté le même, sauf pour une désorientation comprenant quelques degrés. Cette désorientation se montre dans une extension des réflexions lelong des anneaux. Le fait que les réflexions montrent en outre une diffusion radiale est une indication qu'ils existent des différentes distances réticulaires dans divers domaines du monocristal.

Ensuite le cristal fut chauffé dans le vide à 600° C ce qui suffit pour éliminer l'hydrogène et reformer l'état  $\alpha$  (fig. 1c). De cette série de diagrammes il s'en suit que l'absorption de l'hydrogène cause une déformation d'un réseau monocristallin accompagnée d'une désorientation comprenant quelques degrés. Il se produit donc un "break-up" du réseau, probablement dû au fait que successivement des petits domaines sont transformés en phase  $\beta$ , la dilatation du réseau d'un tel domaine causant une désorientation des domaines voisins pas encore transformés.

La figure 2 montre une deuxième série de diagrammes de diffraction de rayons-X obtenus avec un fil monocristallin chargé avec une densité de courant de 8 mA/cm<sup>2</sup> durant 1 heure. Dans ce cas on a réalisé un état montrant les deux phases  $\alpha$  et  $\beta$  simultanément (fig. 2b). Cette photographie montre la coexistence de la phase  $\alpha$  non-déformée (taches assez bien définies) à côté de la phase  $\beta$  déformée et désorientée, en accord avec ce qui fut dit ci-dessus.

La figure 3 présente deux micrographies d'un tel spécimen, lesquelles montrent des bandes gonflées et des lignes de glissement.

D'une analyse de la projection stéréographique toutes les deux sont à peu près parallèles aux plans  $\{111\}$  du réseau atomique. En vertu de mesures de la microdureté Vickers (fig. 3b) il semble probable que les bandes gonflées sont la phase  $\beta$  tandis que les régions contenant des lignes de glissement sont la phase  $\alpha$ . La dureté mesurée (90, pour la phase  $\alpha$  et 99, pour la phase  $\beta$ ) est conforme aux valeurs données par SUGENO et KAWABE [4] pour les deux phases.

Enfin la série de microphotographies représentées dans les figures 4

et 6 provienne d'un cristal cylindrique avec un diamètre de 5 mm. Cette série montre la surface d'un plan (100) après divers stages de chargement de l'hydrogène. C'est à dire après un chargement de respectivement 0, 5, 15 et 120 minutes avec un densité de courant de 280 mA/cm<sup>2</sup>. Les photographies montrent très nettement la formation successive de lignes de fracture ("rifts") (ou lignes de glissement) selon des directions bien définies. Donnée la grandeur du cristal, l'orientation du réseau cristallin et des directions cristallographiques dans la face (100) taillée pouvaient être fixées avec certitude. La figure 5 montre en projection stéréographique

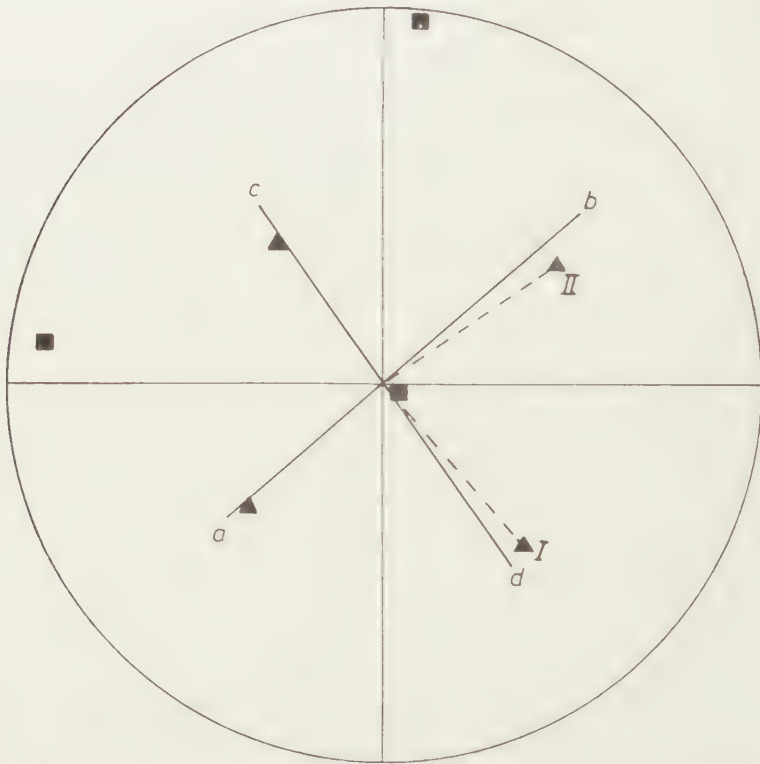


Fig. 5. Projection stéréographique des pôles des plans {100} (■) et {111} (▲), du même cristal des figures 4 et 6. La position du spécimen correspond exactement à celle de la figure 4d. Les lignes *a-b* et *c-d* indiquent les intersections des plans {111} marqués I en II avec le plan de la surface exposée.

(plan de projection parallèle au plan taillé) les pôles des plans {100} et les pôles correspondents des plans {111}. Les lignes indiquent les directions des intersections des plans {111} avec le plan de la projection, donc avec le plan de la surface exposée. En superposant la photographie 4d avec la projection stéréographique, il n'y a pas de doute que les stries visibles après chargement avec de l'hydrogène sont parallèles aux intersections



PLANCHE I

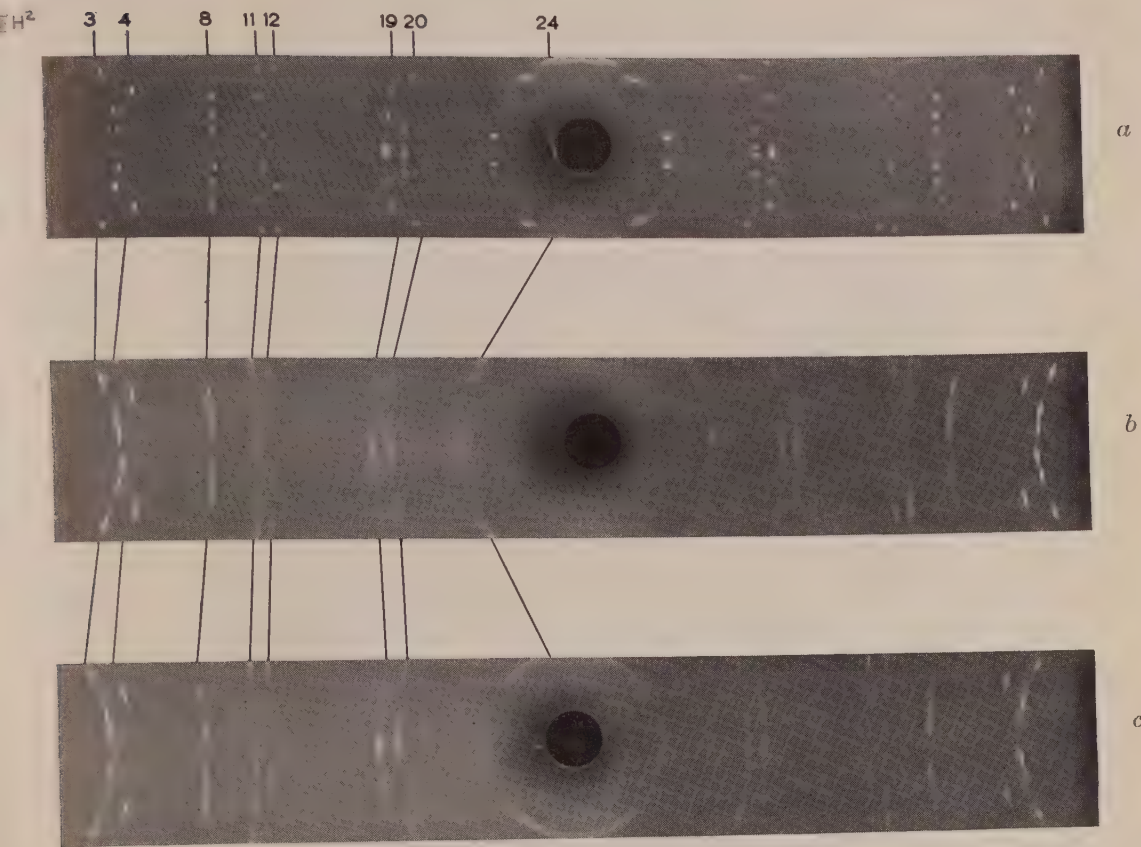


Fig. 1. Diagrammes de cristal tournant, rayonnement  $\text{CuK}\alpha$ , diamètre de la chambre 57,3 mm, montage selon van Arkel. *a*) cristal non chargé (phase  $\alpha$ ); *b*) cristal chargé (phase  $\beta$ ); *c*) cristal après chauffage dans le vide (phase  $\alpha$ ).

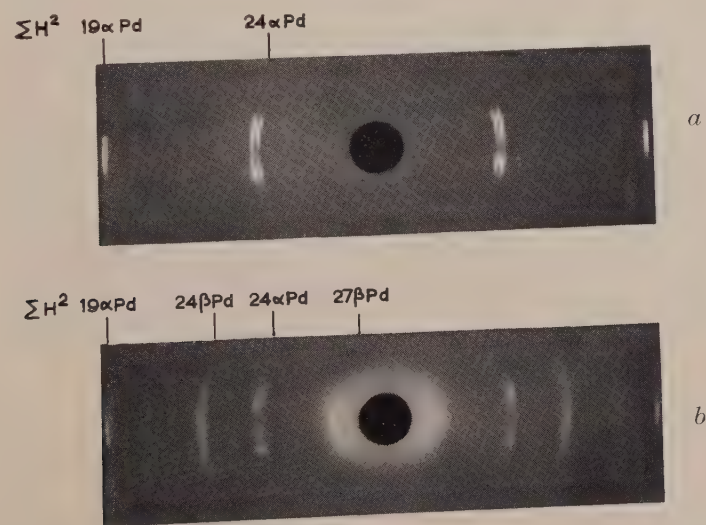


Fig. 2. Diagrammes de cristal tournant, rayonnement  $\text{CuK}\alpha$ , diamètre de la chambre 80 mm, montage selon van Arkel. *a*) cristal non chargé (phase  $\alpha$ ); *b*) cristal chargé (phases  $\alpha$  et  $\beta$ ).

(N.B. la réflexion faible pas indiquée dans la figure *a* est une réflexion du rayonnement  $\text{CuK}\beta$ ).

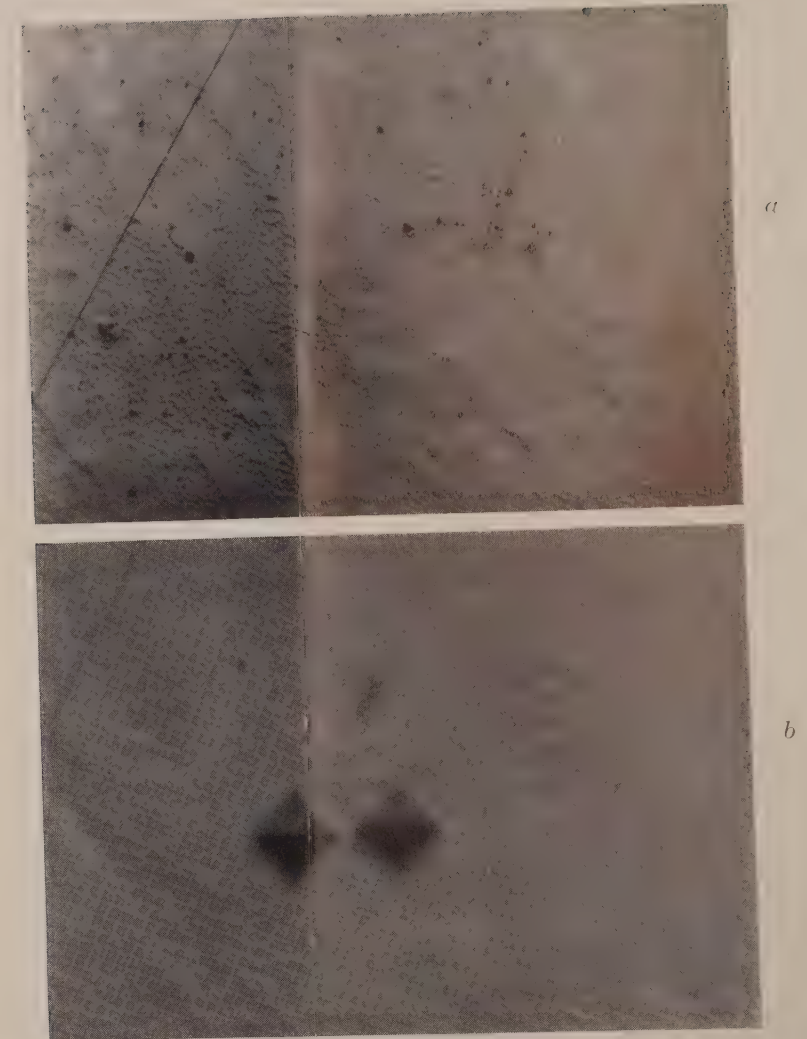


Fig. 3. Micrographies d'un cristal de palladium ( $\phi$  0,5 mm). *a*) cristal chargé montrant des bandes gonflées et des lignes de glissement ( $M \sim 350 \times$ ); *b*) même cristal avec des empreintes de Vickers ( $M \sim 775 \times$ ).





Fig. 4. Micrographies de la surface (plan (100)) d'un monocristal de palladium ( $\phi$  5 mm). ( $M \sim 130 \times$ ). *a*) cristal non chargé; *b-d*) cristal chargé pendant respectivement 5,15 et 120 minutes (Densité de courant 280 mA/cm<sup>2</sup>)

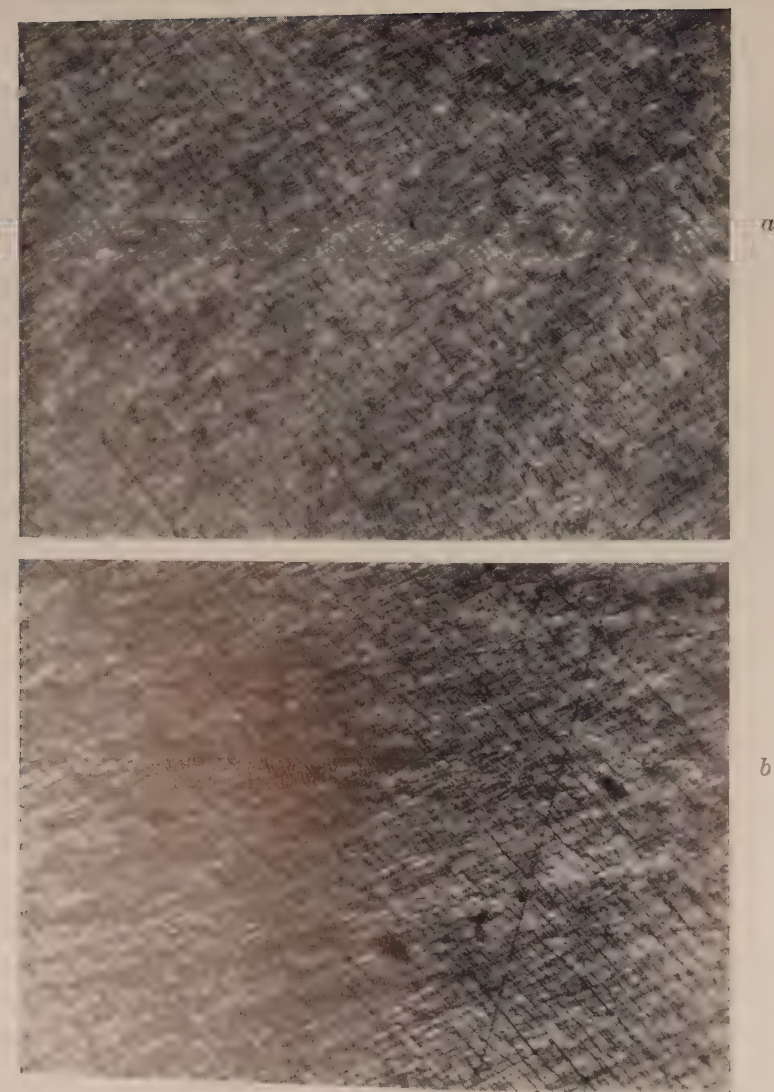


Fig. 5. Micrographies du même cristal. ( $M \sim 130 \times$ ). *a*) avant chauffage dans le vide; *b*) après chauffage dans le vide.

des plans  $\{111\}$  avec le plan taillé. On est porté d'en conclure que les fissures s'écoulent parallèles aux plans  $\{111\}$ <sup>1)</sup>.

La suite des figures 4 montre que, lors du progrès de l'absorption de l'hydrogène, les fissures individuelles ne s'élargissent pas, mais leur nombre augmente, un phénomène analogue à celui que l'on observe lors du glissement d'un cristal où on voit souvent une augmentation du nombre de lignes de glissement plus tôt qu'une intensification de chaque ligne individuelle. (Voir SEEGER [6].)

Il reste à constater qu'après dégagement de l'hydrogène par traitement anodique l'image microscopique reste pratiquement le même (fig. 6), en accord avec ce que montrent les photographies à rayons-X.

Nous nous proposons de continuer ces recherches, tout d'abord pour confirmer les résultats obtenus, mais aussi pour relier ces résultats avec des mesures de dureté.

5. Un autre but de nos recherches comprend la fixation, à l'aide de la diffraction de neutrons, des positions occupées par les atomes de l'hydrogène dans le réseau cristallin. Une telle recherche a été exécutée récemment par WORSHAM, WILKINSON et SHULL [7] pour du palladium pulvérisé et chargé en partant de la phase gazeuse de l'hydrogène. De cette recherche il s'en suit que les atomes d'hydrogène occupent, dans la phase  $\beta$ , des positions octaédriques par rapport aux atomes du palladium (structure du sel de roche). Il serait intéressant de confirmer ce résultat pour le cas que le métal est chargé par voie électrolytique. SCHULDINER, CASTELLAN et HOARE [8], dans une recherche électrochimique du système Pd-H, ont signalé en mesurant les potentiels électrochimiques, que l'hydrogène se comporte différent quand il est introduit en sortant de la phase gazeuse ou par voie électrolytique. À côté de cette question, il nous paraît intéressant de confirmer, et peut être préciser, les résultats obtenus avec des préparations polycristallines en utilisant un monocristal. Pour cette expérience Monsieur le Docteur GOEDKOOP du J.E.N.E.R. à Kjeller, Norvège nous a promis sa coopération.

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<sup>1)</sup> Il faut se réaliser que tous les plans de la zone cristallographique dont l'axe est parallèle à la direction des stries coupent le plan de l'éprouvette parallèlement aux stries observées. C'est le cas par exemple pour certains plans  $\{110\}$ . Cependant, dans ce cas on s'attendrait à observer des stries dans d'autres directions, non présentes sur la photographie. Du reste, les plans  $\{111\}$  sont les plans les plus denses, c'est à dire leur distance réticulaire est la plus grande dans le réseau du palladium, ce qui le rend probable que les fissures sont parallèles à ces plans spéciaux.

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## THEORETICAL PHYSICS

# EHRENFEST'S CONTRIBUTIONS TO THE DEVELOPMENT OF QUANTUM STATISTICS. I

BY

MARTIN J. KLEIN

(Communicated by Prof. S. R. DE GROOT and Prof. C. J. GORTER at the meeting of November 29, 1958)

### ABSTRACT

This paper is a study of the part played by Ehrenfest in the development of the quantum statistics. The first part deals with his papers (from 1911 to 1925) which brought out the lack of independence of photons which was implied by the Planck distribution law. The second part discusses the Ehrenfest-Trkal paper of 1920 which showed how to treat dissociating systems and which handled the question of the dependence of the entropy on the number of particles. The last section of this second part discusses the work of Uhlenbeck and Ehrenfest in clarifying the interpretation of the Bose-Einstein and the Fermi-Dirac statistics. Throughout the paper Ehrenfest's contributions are considered against the background of the developing ideas of the times in which they were written.

### I. *Introduction*

In 1933 PAULI concluded his obituary notice [1] of Paul Ehrenfest with these words. "If we review Ehrenfest's scientific work again, it appears to us as a living testimony to the lasting truth: objective criticism in science, no matter how sharp, is always stimulating and fruitful, if only it is thought through consistently and completely". There is indeed no physicist whose work illustrates as well as Ehrenfest's does the function of criticism in science, not only because of the force and fruitfulness of his critical writings, but even more because his work was centered around his critical activity. It is unusual that a physicist of the first rank makes his most important contributions to the development of his science through his criticism, yet this is true of Ehrenfest. EINSTEIN wrote [2] that, "His stature lay in his unusually well developed ability to grasp the essence of a theoretical notion, to strip a theory of its mathematical accouterments until the simple basic idea emerged with clarity ... he always brought clarity and acuteness into any discussion".

The light that Ehrenfest's work cast on physical theory frequently led to the identification of the dark corners, the obscurities, which upon investigation proved to be the positions where progress could be made. His critical method is properly characterized by the adjective Socratic. It is well known that the growth of science comes at least as much from formulating the right questions as it does from answering them, and

putting questions was one of Ehrenfest's specialties. Many of his papers have questions for their titles and one famous one is devoted solely to basic questions on quantum mechanics. It follows that Ehrenfest's work consisted, so to speak, of participation in the general discussion of the urgent current problems of physics, and his criticism is therefore to be distinguished from the work of those critics whose aim is to reformulate established theory in the most elegant or logically economical way.

In this paper I want to examine Ehrenfest's contributions to the development of one of the radically new ideas in modern physics, the idea that particles of a given kind are not independent individuals, that, as SCHROEDINGER put it [3], "The unsuspected epithet 'this' is not quite properly applicable to, say, an electron, except with caution, in a restricted sense, and sometimes not at all". This concept, whose precise meaning is expressed in the Fermi-Dirac and Bose-Einstein statistics, is not due to Ehrenfest, but his work did much to clear the ground for its growth and to assist its development.

The notion of the elementary particle as an independent individual became problematic with Planck's introduction of quanta and with the elaboration of the idea of photons by Einstein. The problems associated with the statistics of photons and the theory of radiation were considered by Ehrenfest in a number of papers from 1911 on, and these papers are discussed, with the necessary background on the contemporary status of the problems, in Section II below.

There were also problems in the statistical mechanics of material particles, and in particular in the calculation of the dependence of the entropy of a gas on the number of particles in the gas. Here too the question of particle identity was essential, and Ehrenfest and Trkal in an important paper clarified just what the difficulty was. This paper also served to rule out a number of apparently easy but inadequate methods of by-passing the real issue. This work is discussed in Section III below.

Finally, in the last section of this paper, I discuss the part played by Ehrenfest and Uhlenbeck in illuminating the meaning of the quantum statistics at the time of its development, and in showing how the quantum statistics deepened our understanding of the problems previously considered.

## II. *The Statistics of Photons*

It has already been pointed out that the difficulties in the concept of an elementary particle, which were not finally resolved until the development of wave mechanics, began in the interpretation of Planck's quantum theory. More specifically, it was the differences in the status accorded to quanta by Planck himself in the theory of heat radiation and by Einstein and others in their applications of the quantum theory which gave rise to the question, first formulated clearly by Ehrenfest, are the quanta independent particles? Before we examine Ehrenfest's analysis of this

question it will be useful to review briefly the background against which this analysis was made, and we shall begin with a summary of Planck's original derivation of the distribution law for heat radiation [4].

Planck's method for determining this distribution law rests essentially on Kirchhoff's result that the distribution depends only on the temperature and not on the nature of the bodies with which the radiation interacts in coming to thermodynamic equilibrium. Planck considers linear harmonic oscillators as the simplest possible bodies which can absorb and emit radiation. Using arguments based on the classical electromagnetic theory of emission and absorption, and using the requirement that, at equilibrium, absorption and emission rates must be equal, Planck finds that the average energy of the thermal radiation in the frequency range  $\nu$  to  $\nu + d\nu$ , per unit volume,  $E_\nu d\nu$ , is given by

$$(1) \quad E_\nu d\nu = \frac{8\pi\nu^2}{c^3} U d\nu$$

where  $U$  is the average energy of a linear oscillator of frequency  $\nu$ , at the common temperature  $T$  of the oscillators and the radiation. Planck's problem is then to find the average energy  $U$  of the linear oscillator, and it is here that the quantum hypothesis enters. To find this average energy  $U$ , Planck considers  $N$  oscillators of frequency  $\nu$ , and assumes that the total energy shared by these oscillators consists of a number of discrete elements of energy (or quanta) each of magnitude  $\epsilon$ , i.e. the total energy is  $P\epsilon (=NU)$ . Planck's method is to compute the entropy of this system of oscillators, which share the energy  $P\epsilon$ , by using Boltzmann's basic relationship between the entropy  $S$  and  $W$ , the number of ways in which the energy can be distributed among the oscillators. This relationship is

$$(2) \quad S = k \ln W$$

and  $W$  is also called the thermodynamic probability<sup>1)</sup>. Now  $W$  is readily computed since it is the number of ways in which  $P$  objects can be distributed among  $N$  containers, where we do not care which objects are in particular containers, but only how many objects are in each container. Combinatorial analysis leads to the following expression for  $W$ ,

$$(3) \quad W = (P + N - 1)! / P!(N - 1)!$$

The development is now straightforward. This expression for  $W$  is substituted into Eq. (2), the factorials are simplified with the help of Stirling's formula and  $P$  is replaced by  $NU/\epsilon$ . This gives an expression for  $S$  as a function of  $U$ , and making use of the thermodynamic definition of the temperature

$$(4) \quad T^{-1} = \frac{\partial S}{\partial U}$$

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<sup>1)</sup> Boltzmann never used this relationship as an equation, but only as a proportionality. Planck was the first to write "Boltzmann's equation" in this form and to evaluate the constant  $k$ .



one readily obtains the equation

$$(5) \quad U = \epsilon / (e^{\epsilon/kT} - 1).$$

Wien's displacement law is now introduced. This law leads to the condition that  $\epsilon$  be proportional to  $\nu$ , the frequency of the oscillator,

$$(6) \quad \epsilon = h\nu.$$

Finally, introducing Eq. (1) which relates  $U$  to the energy distribution for the radiation in equilibrium with the oscillator at temperature  $T$ , one obtains the Planck law for the average energy density  $E_\nu d\nu$  of the radiation in this frequency range,

$$(7) \quad E_\nu d\nu = \frac{8\pi\nu^2 d\nu}{c^3} \frac{h\nu}{e^{h\nu/kT} - 1}.$$

It should be noted that Planck assumed that an oscillator of frequency  $\nu$  can exchange energy only in quanta  $h\nu$ , and that what is important in the counting procedure for obtaining  $W$  is just the number of such quanta possessed by a given oscillator. No quantization assumptions were applied directly to the radiation.

Let us now examine the way in which the quantum concept was developed by Einstein. In his first paper [5] in 1905 EINSTEIN developed a "heuristicly useful point of view" on the behavior of radiation, namely, that radiation behaves as if it consists of quanta of energy of magnitude  $h\nu$ . In this same paper Einstein applied this granular picture of radiation to the photo-electric effect and Stokes' law for fluorescence, and in later papers [6] to photo-chemical processes and to the specific heats of solids, in all cases with great success.

In all of this work Einstein treats the radiation as actually consisting of independent quanta of energy. Einstein's justification for this point of view is given first in the 1905 paper, and it is necessary for us to examine this justification. What Einstein does is to calculate the contribution to the entropy of thermal radiation which is due to oscillators of a given frequency  $\nu$ . This calculation is made, however, only for the case that  $h\nu/kT \rightarrow 1$ , when the Planck law assumes a limiting form first given by Wien and experimentally well confirmed

$$(8) \quad E_\nu d\nu = \frac{8\pi h\nu^3 d\nu}{c^3} e^{-h\nu/kT},$$

and is carried out with the help of Eq. (4). Einstein then inverts Eq. (2) and uses it to draw conclusions about the probabilities of various states of the system, i.e. to obtain  $W$  from the macroscopic or empirical form of  $S$ <sup>1)</sup>.

<sup>1)</sup> The inversion of the Boltzmann relation, Eq. (2), by Einstein was by no means a trivial step. Einstein was concerned to give the thermodynamic probability  $W$  a direct physical interpretation as the frequency of occurrence of, or relative

In particular Einstein shows that the probability  $W'$  that all the radiation will be located in a sub-volume  $V'$  of  $V$  is given by the equation

$$(9) \quad W'/W = e^{(S' - S)/k} = (V'/V)^{E/h\nu}$$

where  $E$  is the energy of this monochromatic radiation. Since, however, the relative probability that all  $n$  molecules of an ideal gas in a container of volume  $V$  will be found in a sub-volume  $V'$  is  $(V'/V)^n$ , Einstein concludes that the radiation of frequency  $\nu$  behaves as if it were a gas of  $n$  independent particles (photons), each of energy  $h\nu$ . (This conclusion assumes  $\nu/T$  has a value such that Wien's law holds.)

Einstein's bold extension of the quantum theory to the properties of radiation in vacuum led to a general discussion of the relationships between this new work and Planck's original use of quanta [8, 9]. Planck expressed his own views at the first Solvay Conference in 1911, where the quantum theory was the principal subject discussed [10]. PLANCK stated [11] that he held a middle ground between the absolute conservatives such as Jeans, who were still trying to explain black body radiation without making any use of quanta, and the radicals, led by Einstein, who were treating radiation as really composed of discrete quanta of energy. The discussions at this Solvay Conference give a vivid picture of the state of thought on quantum theory at this time.

Ehrenfest was still in Russia in 1911 and he was not a member of the Solvay Conference. His paper [12], published just before the Conference, was apparently not mentioned there, but it must be considered as a contribution to the general discussion of quanta then in progress. The paper is entitled, characteristically enough, with the question, *Which features of the hypothesis of light quanta play an essential role in the theory of heat radiation?* Ehrenfest was concerned with identifying those aspects of the quantum theory which could be considered as definitely established, i.e. as required in order to explain the special features of black body radiation. Those features of the quantum theory which went beyond this established minimum would, as he put it, probably have their fate decided in the new fields of application opened by Einstein.

Ehrenfest's arguments determine the restrictions imposed by the known characteristics of black body radiation on the weight function  $\gamma(\nu, E)$  used in any statistical theory of radiation. This weight function is defined so that  $\gamma(\nu, E)dE$  is the a priori probability that an oscillator of frequency  $\nu$  will have its energy in the interval  $E$  to  $E + dE$ . (This weight function generalizes Boltzmann's assumption that regions of equal volume in phase

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time spent in, the state in question. Because Einstein treated both  $S$  and  $W$  as, so to speak, observable quantities, and used  $S$ , obtained from thermodynamics, to obtain information about the probability  $W$ , he was able to develop the fluctuation theory and apply it to the Brownian Motion [7]. (Einstein was in fact disturbed by Planck's willingness to ignore the problem of what  $W$  really means in the theory of thermal radiation. See reference 10, pp. 115, 436 et seq.)

space are to be equally weighted). Ehrenfest's analysis is based on Wien's displacement law and on the empirical requirements that the spectral distribution must vanish sufficiently rapidly at the two limits, zero frequency and infinite frequency. These assumptions together with the statistical interpretation of the entropy, Eq. (2), lead, after detailed analysis, to the following restriction on the weight function,  $\gamma(\nu, E)$  must be a function of the combination  $\nu/E$  only. Furthermore, the *particular* energy  $E=0$  must have a weight equivalent to that normally associated with an energy *interval*, and the energy interval from  $E=0$  up to  $E=\alpha\nu$ , where  $\alpha$  is a constant, must have zero weight. In other words the oscillator must behave as if it had a threshold of stimulation which must be exceeded before it can absorb energy. (Note that these results do not require the particular form of the Planck distribution, but instead require only that there is no ultra-violet catastrophe). (I must also note that the use of the Wien displacement law here was one of the seeds which later blossomed in Ehrenfest's adiabatic hypothesis.)

Ehrenfest then turns to the assumptions underlying Einstein's use of quanta, e.g. in the theory of the photoelectric effect, and finds that there are three. An oscillator of frequency  $\nu$  can have its energy equal to  $n h \nu$  where  $n$  is an integer. These energies come about through the juxtaposition of elementary, independent quanta of energy  $h\nu$ . These quanta not only act as "atoms" in absorption and emission processes, but they also exist separately in space free of matter.

Now as we mentioned above in summarizing Planck's views on the status of quanta in 1911, the prevailing opinion was that Einstein's new assumption was the third of those just listed, the existence of independent quanta in free space. Ehrenfest points out, however, that the second of Einstein's assumptions is itself new and goes beyond what is required for the theory of heat radiation. (The first is a generalization of what *is* required and was of course, already used by Planck.) Furthermore, the assumption that the quanta act as independent particles is *not* compatible with the Planck radiation law Eq. (7), but it leads instead to the Wien Law, Eq. (8). (This last point is, in a way, not so surprising if one re-examines Einstein's arguments justifying his quanta since, as we have seen, these arguments assume the Wien law. The Wien law, Eq. (8), is in fact just what one obtains by applying the Boltzmann statistical mechanics to the quanta).

Ehrenfest's remark that independent quanta do not give the Planck law, which is not proved directly in his 1911 paper, was not universally accepted at first, despite the fact that others (Joff  [13], NATANSON [14]) had already pointed out that, in order to obtain Planck's law, one must assume that the quanta can form aggregates of energies  $2h\nu, 3h\nu, \dots$ , and that the total energy is shared among the free quanta and these aggregates. It is fair to say that neither Joff  nor Natanson made the point absolutely clear. There is an interesting controversy in the Physi-



kalische Zeitschrift for 1914 between KRUTKOW [15] and WOLFKE [16] dealing with just this matter—whether or not independent quanta lead to the Planck law. Krutkow develops Ehrenfest's remark and proves explicitly that independent quanta lead to Wien's law for radiation when the usual Boltzmann statistics are used. Wolfke on the other hand insists that if the quanta *exist* independently one obtains Planck's law, but if in addition the quanta are spatially independent, as in Eq. (9) above, only then does one obtain the Wien law. There is no record of ultimate agreement in this exchange which rested on the ambiguity of the word "independent".

The subject was more fully clarified in another paper [17] by EHRENFEST (with KAMERLINGH-ONNES) in 1915 on *A Simplified Derivation of the Combinatorial Formula which underlies Planck's Radiation Theory*. The formula in question is Eq. (3) above to which we have already called attention. Ehrenfest points out that the derivations of Eq. (3) in texts on combinatorial analysis are rather long and give no real insight into the structure of the result, in particular into the appearance of the  $(N-1)!$  in the denominator [18]. The new derivation is exceedingly lucid and does make the result intuitively clear, and it is now the one usually given. The derivation can be considered as a typical Ehrenfest pedagogical point bringing out the essence of the situation.

The argument runs as follows. The problem is to find the number of distinct ways in which  $P$  objects (energy units) can be placed in  $N$  containers (oscillators) where only the number of objects (energy units) in each container (oscillator) is of importance. For illustrative purposes suppose  $N=4$ ,  $P=7$ . Then a particular distribution is symbolized by the expression

$$I \epsilon \epsilon \epsilon \epsilon O \epsilon \epsilon O O \epsilon I$$

where each  $\epsilon$  represents a quantum, the symbols  $O$  separate the quanta in successive oscillators, and the symbols  $I$  are the "fixed boundaries". Thus, in the illustration, there are four quanta assigned to the first oscillator (it has energy  $4\epsilon$ ), two quanta assigned to the second, none to the third and one to the fourth. In general there are  $P$   $\epsilon$ 's and  $(N-1)$   $O$ 's (or separations) in such a symbolic distribution. There are  $(P+N-1)!$  permutations of the  $P$   $\epsilon$ 's and  $(N-1)$   $O$ 's, and we must divide by the number of permutations of the  $P$   $\epsilon$ 's,  $P!$ , and by the number of permutations of the  $(N-1)$   $O$ 's,  $(N-1)!$ , to obtain the number of distinct distributions. When we carry out these simple operations we obtain Eq. (3).

The important conclusion to be drawn from the insight into Eq. (3) which this new derivation affords is emphasized by Ehrenfest in the Appendix to this 1915 paper. It is simply that Planck's combinatorial formula is incompatible with Einstein's quanta. That is, in Einstein's quantum theory the change in entropy which occurs when  $P$  quanta are allowed a volume of  $N_2$  cm<sup>3</sup> when formerly they occupied a volume of  $N_1$  cm<sup>3</sup> is  $S = k \ln (N_2/N_1)^P$ .

In the Planck theory, on the other hand, the entropy change which occurs when  $P$  quanta are distributed over  $N_2$  oscillators rather than  $N_1$  oscillators is

$$S = k \ln \frac{(P+N_2-1)!}{P!(N_2-1)!} - k \ln \frac{(P+N_1-1)!}{P!(N_1-1)!};$$

in other words the quanta as used in Planck's theory are not the independent particles of Einstein's picture; they are not "separate from each other".

(It may be mentioned at this point that if Planck's derivation of the radiation law, Eq. (7) is repeated with  $N^P$  used for  $W$  instead of the expression given in Eq. (3), the Wien Law is *not* obtained. It is necessary to take  $W$  equal to  $N^P/P!$  in order to obtain the Wien law directly. When  $W$  is large compared to  $P$  the two forms for  $W$ , Eq. (3) and  $N^P/P!$  are almost equal. We shall discuss this point later on.)

We may observe that these discussions make it evident that a particle picture for radiation must use particles which are fundamentally different from those previously studied in science. That is, these particles or quanta must be counted in a new way. At the time the work we have been examining was done, all of this formed a puzzle, a puzzle not resolved until the development of wave mechanics which gave a real understanding of the nature of the new counting or new statistics.

The paradoxical nature of photons was brought out perhaps even more clearly by the discussions of the fluctuations in energy in thermal radiation [19]. This fluctuation calculation was made by Einstein in 1909 and it was based on the Planck distribution law, in contradistinction to the 1905 calculation, discussed above, which was based on the Wien distribution law. The calculation can be described very simply. Statistical thermodynamics tells us that a system of volume  $V$  in contact with a heat bath, which keeps it in thermal equilibrium at temperature  $T$ , does not have a fixed value for its energy  $E$ . Instead this energy will fluctuate about its mean value  $U$ ,  $U$  being equal to the thermodynamic internal energy. The mean square fluctuation in energy is given by

$$(10) \quad \overline{\epsilon^2} = \overline{(E-U)^2} = kT^2 \left( \frac{\partial U}{\partial T} \right)_V.$$

Einstein applies this general expression to the fluctuation of that part of the energy of black body radiation whose frequencies lie in the interval  $\nu$  to  $\nu+d\nu$ . For this part of the energy  $U$  is given by  $E_\nu d\nu$  of Eq. (7) and a simple calculation leads to the expression

$$(11) \quad \overline{\epsilon^2} = h\nu + \frac{c^3 U^2}{8\pi \nu^2 V d\nu}.$$

The second term in Eq. (11) is the fluctuation which one would obtain if the radiation consisted of classical electromagnetic waves and the fluctuations came about by the interference of these waves. This was

argued by Einstein, and shown in detail by LORENTZ [20]. The first term in Eq. (11), absent in a purely classical calculation, is precisely what one would obtain if the radiation consisted of particles each of which had energy  $h\nu$  [21]; (in that case the fluctuation in the number  $n$  of particles in a small volume is given by  $\overline{(n - \bar{n})^2} = \bar{n}$ . If each particle has energy  $h\nu$  then the first term of Eq. (11) follows directly with  $\epsilon = nh\nu$ ,  $U = \bar{n}h\nu$ ).

The conclusion to be drawn is that the fluctuation calculation shows the coexistence, in some sense, of both wave and particle properties in radiation, and these combine in the manner of independent causes of fluctuations. It is also suggestive that the first term alone appears when Planck's law is replaced by Wien's law, i.e. when  $h\nu/kT \gg 1$ , and the second term alone appears when Planck's law is replaced by the classical Rayleigh-Jeans law, i.e. when  $h\nu/kT \ll 1$ . Eq. (11) may be properly described as the first appearance of the wave-particle duality which has become so important in the interpretation of quantum mechanics.

Einstein's fluctuation result was discussed from various points of view in the years following 1909. It was pointed out by DE BROGLIE [22] and BOTHE [23], for example, that the complete formula could be obtained from a particle picture for radiation if particles of energies  $h\nu$ ,  $2h\nu$ ,  $3h\nu$ , ... shared the energy in an appropriate way.

In 1925, on the eve of the birth of quantum mechanics, and in fact after the appearance of Heisenberg's first paper, EHRENFEST published a discussion [24] of the fluctuations of thermal radiation. This paper again points up the wave-particle paradoxes associated with the Einstein fluctuation formula. In particular, Ehrenfest puts the problem this way: Planck's radiation law can be derived by taking the electromagnetic waves in an enclosure, quantizing their energies, and treating them by Planck's "energy level statistics". Yet Einstein's fluctuation formula, which is an inevitable thermodynamic consequence of the Planck law, includes a term which cannot be understood from the wave picture. How does this come about? (There is an interesting personal note at the beginning of this paper in which Ehrenfest points out that it was his wife who remarked on the puzzle in this particular way.)

The argument of the paper is intended to clarify the point at issue by elaborating on what had already been pointed out by ORNSTEIN and ZERNIKE [25]: Einstein's calculation assumes that the entropies of different volume elements of the enclosure are strictly additive, and therefore the fluctuations in such elements are independent, and such independence is incompatible with fluctuations due to interfering waves. Ehrenfest's conclusion is that for Einstein's formula to be valid the fluctuation cannot be computed as arising from interfering waves.

Ehrenfest carries out the analysis for a one dimensional example, treating the waves on a string with fixed end points. Let  $e(t)$  be the energy of a sub-region of the string at time  $t$  (in the frequency range  $\nu$  to  $\nu + d\nu$ ). Ehrenfest distinguishes between two kinds of averaging:  $\langle e(t) \rangle = \eta$  is the



average of  $e(t)$  over a standard time period, say a day, during which  $e(t)$  varies because of wave interference. The value of  $\eta$  will depend on the initial conditions that day. The second averaging process comes about because "once a day at midnight" the string is coupled to a heat bath at temperature  $T$  to destroy phase relations, or as Ehrenfest puts it, a Planck lottery is held. This averaging process is denoted by  $[e(t)] = e_0$ . Ehrenfest then shows that the lottery average of this daily time average has a fluctuation given by the expression

$$(12) \quad [ \langle (e(t) - \eta)^2 \rangle ] = e_0^2 \left( \frac{1}{z} - \frac{1}{Z} \right)$$

where  $e_0 = [e(t)]$  is the quantity equivalent to the thermodynamic average energy  $U$ .  $Z$  is the number of modes of the string in the interval  $v$  to  $v + dv$  ( $8\pi v^2 V dv / c^3$  in the electromagnetic three dimensional case) and  $z$  is  $Z$  times the fraction  $v/V$  where  $v$  is the volume (length) of the sub-region.

When, on the other hand, the fluctuations of the lottery average are computed directly Ehrenfest obtains the expression

$$(13) \quad [(e(t) - e_0)^2] = e_0^2 \left( \frac{1}{z} + \frac{1}{Q} \right)$$

where  $Q$  is equal to  $(e_0/h\nu) (V/v)$ .

In both cases, Eq. (12) and (13), as  $v/V$  goes to zero, that is as the sub-region whose energy fluctuations are considered becomes relatively small, the fluctuation result approaches the second or wave term in the Einstein formula, Eq. (11). In neither case, however, is the relative fluctuation proportional to  $v$  (except in the limit where  $v/V$  is negligible), so that Einstein's additivity assumption is not fulfilled.

It is perhaps appropriate to mention here that this very clear exposition of radiation fluctuation theory was first presented by Ehrenfest as a lecture in Goettingen, and this lecture is acknowledged by BORN, HEISENBERG and JORDAN as an important influence on their quantum mechanical treatment of the problem a few months later [26].

The situation was not fully clarified until many years later with the development of the new quantum mechanics. Despite a certain amount of controversy it turned out that the Einstein equation for the fluctuations is correct, although it takes some careful quantum mechanical arguments, to give a satisfactory discussion of the "mechanism" of the fluctuations [27].

This completes the account of the problem of photon statistics for the present. The important papers by Bose and Einstein in 1924-25 are discussed below, in Section IV, in connection with Ehrenfest's contributions to the new quantum statistics.

*(To be continued)*

EHRENFEST'S CONTRIBUTIONS TO THE DEVELOPMENT OF  
QUANTUM STATISTICS. II

BY

MARTIN J. KLEIN

(Communicated by Prof. S. R. DE GROOT and Prof. C. J. GORTER at the meeting of November 29, 1958)

III. *The Statistics of Atoms and Molecules*

The problem of particle identity and independence also gave rise to difficulties in fields quite distinct from the theory of radiation. Thus, in 1920 EHRENFEST [28] called attention to a "dark point" in the statistical mechanics of gases, a "dark point" connected with just this problem. The obscurity in question came in understanding the dependence of the entropy of a gas on  $N$ , the number of its molecules. This question of the dependence of the entropy on  $N$  had come up in the theoretical treatment of such problems as chemical equilibrium in gases, where the numbers of molecules of the various chemical species are not constant. Previous derivations of the chemical constants of gases had been either indirect or intrinsically obscure, so that one could not see the essential features of the situation. Ehrenfest and his student Trkal showed how to treat the problem directly, in a straight forward way, and calculated the chemical constants without arbitrary assumptions on the  $N$  dependence of the entropy. They also showed that all previous direct calculations of the chemical constants became obscure when, by one artifice or another, a factor of  $(N!)^{-1}$  was introduced into the entropy so as to make the entropy an extensive property, as seemed to be required for the thermodynamic use of the entropy. This apparent need for an extensive entropy led to an interesting exchange between Ehrenfest and Planck which I shall discuss later in this section. The relationships between the  $(N!)^{-1}$  and the new quantum statistics are discussed in Section IV.

In order to point out just what the Ehrenfest-Trkal paper accomplished, I shall review the theory of dissociation equilibrium as it existed before their paper, and then describe the structure of their treatment.

Consider a simple dissociation reaction  $A + B \rightleftharpoons AB$ . The condition for chemical equilibrium at given pressure  $P$  and temperature  $T$  can easily be obtained by purely thermodynamic arguments. The result can be written in the form

$$(14) \quad \ln(x_A x_B / x_{AB}) = -\ln P - D/RT + (\Delta C_P/R)(1 + \ln T) + \Delta S^0/R,$$

where  $x_A$  is the molar concentration of  $A$ ,  $D$  is the dissociation energy per mole of gas  $AB$ ,  $\Delta C_P$  is  $C_P^A + C_P^B - C_P^{AB}$  and  $\Delta S^0$  is  $S_A^0 + S_B^0 - S_{AB}^0$ . The  $S_0$  are the molar entropy constants of the gases defined by equations of the type

$$(15) \quad S_A = C_P^A \ln T - R \ln P + S_A^0,$$

and pure thermodynamics cannot calculate the values of the  $S^0$ . The problem for statistical mechanics is then to determine the entropy constants or rather the combination  $\Delta S^0$ . Two different approaches were used before Ehrenfest and Trkal. The first, used by SACKUR [29] and TETRODE [30], was to make a direct calculation of the separate  $S^0$ 's and then to combine them to obtain the explicit equilibrium condition. The second method, used by STERN [31] also calculated the  $S^0$ 's but by an indirect argument involving the properties of the corresponding solid near absolute zero. This indirect method, undoubtedly correct, will not be discussed further here because of the detour through the solid phase which it introduces into the problem. The calculation of the  $S^0$ 's runs into the difficulties, mentioned above and discussed in detail below, over the dependence of the entropy on the particle numbers. Consequently Ehrenfest and Trkal attacked the equilibrium problem directly: that is, they applied statistical mechanics to the mixture of reacting gases  $A$ ,  $B$  and  $AB$  and calculated the equilibrium state and the combination of constants  $\Delta S^0$ .

Let us outline their argument in more modern language than they used. Suppose that we have a vessel of volume  $V$  at temperature  $T$  which contains  $N_A$   $A$  atoms and  $N_B$   $B$  atoms which can exist either as free atoms or as  $AB$  molecules. If we knew that there were  $n_A$  free  $A$  atoms,  $n_B$  free  $B$  atoms and  $n_{AB}$   $AB$  molecules the partition function  $Z(n_A, n_B, n_{AB})$  would be given by the equation

$$(16) \quad Z(n_A, n_B, n_{AB}) = [z_A^{n_A} z_B^{n_B} z_{AB}^{n_{AB}}] \left[ \frac{N_A! N_B!}{n_A! n_B! n_{AB}!} \right].$$

Here  $z_A$ ,  $z_B$ ,  $z_{AB}$  are the partition functions for single molecules of the three species; their explicit forms are listed below. The first factor is the partition function for the mixture of three gases of known composition and the second factor is the number of ways in which the  $N_A$   $A$ 's and  $N_B$   $B$ 's can be grouped to give  $n_A$ ,  $n_B$  and  $n_{AB}$  molecules of the three kinds. Note that this second factor, which gives the dependence on particle numbers, is quite straightforwardly obtained, and that the particles are counted in the normal classical way.

Now we do not in fact know the values of  $n_A$ ,  $n_B$  and  $n_{AB}$ . These parameters may take on all values subject only to the restrictions on the total numbers of  $A$ 's and  $B$ 's,

$$(16') \quad n_A + n_{AB} = N_A; \quad n_B + n_{AB} = N_B.$$

The correct partition function  $Z$  is the sum of  $Z(n_A, n_B, n_{AB})$  over all values



of the  $n$ 's consistent with the restrictions of Eq. (16'). It is not necessary to evaluate this sum since, by the usual arguments of statistical mechanics [32], the sum is well approximated by its largest term, which is always overwhelmingly the largest term. It is easy to maximize  $Z(n_A n_B n_{AB})$  subject to the constraint of Eq. (16'), and we obtain as our result that  $Z$  is equal to  $Z(n_A^* n_B^* n_{AB}^*)$  where the  $n^*$  are determined by the condition

$$(17) \quad n_A^* n_B^* / n_{AB}^* = z_A z_B / z_{AB}$$

together with Eq. (16'). The  $n^*$  determine the mole numbers of the different species  $A$ ,  $B$  and  $AB$  at equilibrium.

To obtain the equilibrium condition explicitly we need to evaluate the  $z$ 's. If we treat the translations of all the molecules and the rotations of the diatomic molecules as classical motions, and if we assume that the vibrations of the diatomic molecules are unexcited, then we can write the equations

$$(18a) \quad z_A = V(2\pi m_A kT)^{3/2} / h^3$$

$$(18b) \quad z_{AB} = V(2\pi (m_A + m_B) kT)^{3/2} kT (8\pi^2 I) e^{-\chi/kT} / h^5$$

where  $m_A$ ,  $m_B$  are the atomic masses,  $I$  is the moment of inertia of the  $AB$  molecule about its mass center and  $\chi$  is the binding energy of the  $AB$  molecule [33].

Note that each molecular partition function contains a factor of  $h^{-f}$  where  $f$  is the number of excited degrees of freedom of the molecule. (This introduction of factors of  $h$  could not be *fully* justified at the time this work was first done, but these factors were not the point at issue.)

If the partition functions of Eq. (18) are used in Eq. (17) and the result is simplified, the equilibrium condition can be rewritten in the explicit form

$$(19) \quad \ln(x_A x_B x_{AB}) = -\ln P + \chi/kT + \frac{3}{2} \ln T + \ln \left[ \frac{h^{5/2}}{h} \left( \frac{m_A \cdot m_B}{m_A + m_B} \right)^{3/2} \frac{1}{(8\pi)^{1/2}} \right].$$

One can see directly that Eq. (19) has the same form as Eq. (14); the factor  $3/2$  which multiplies  $\ln T$  is equal to  $\Delta C_P/R$  in the present case, and the dissociation energy per mole,  $D$ , is the negative binding energy per particle,  $-\chi$ , times Avogadro's number,  $R/k$ . The condition for chemical equilibrium has been found explicitly, and the combination  $\Delta S^0$  has been obtained *without* trying to or needing to compute the  $S^0$  individually.

Let us turn now to the question of the dependence of the entropy on the number of particles. It will be noted that this dependence was not needed for the Ehrenfest-Trkal calculation of chemical equilibrium, but in the interesting concluding section of their paper they discuss the difficulties in other calculations which do require the  $N$  dependence of the entropy. All such calculations have the following structure: the  $S^0$  are calculated individually by statistical mechanics, and are then combined

with the thermodynamic Eq. (14) to determine the equilibrium condition explicitly. It is clear from an inspection of Eq. (14), or from its simplified form as the law of mass action,

$$x_A x_B / x_{AB} = f(P, T),$$

that the  $S^0$  must not depend on the concentrations. In other words, to obtain a meaningful form for the equilibrium condition the entropy expression for the individual gas must be extensive, when the calculation uses this mixed thermodynamic-statistical approach.

The difficulty, however, is that a straightforward calculation does *not* give an extensive entropy function. To see this in detail we need merely point out that for a gas composed of  $N$  independent particles, an ideal gas, the partition function  $Z$  must be of the form

$$(20) \quad Z = z^N$$

where  $z(V, T)$  is the single particle partition function of Eq. (18). This is a direct consequence of the independence of the particles. But the entropy derived from Eq. (20) is not an extensive function. For a monatomic gas, for example, it is given by the equation

$$(21) \quad S = k \ln Z + kT \frac{\partial \ln Z}{\partial T} = Nk \ln V + \frac{3}{2} Nk - 3 Nk \ln \lambda$$

where  $\lambda$  is  $h(2\pi m kT)^{-1/2}$ . The entropy so specified will *more* than double when both  $N$  and  $V$  are doubled, and so it cannot be used to determine the condition for chemical equilibrium.

Inspection of Eq. (20) and (21) shows that if  $Z$  were given by  $z^N/N!$  rather than by  $z^N$ , then  $S$  would indeed be extensive. SACKUR [29], TETRODE [30], PLANCK [34] and others had advanced arguments for including such an  $N!$ , but, as Ehrenfest pointed out, these arguments were simply not cogent. Furthermore, Ehrenfest posed the question as to whether thermodynamics really requires an extensive entropy. As he put it [28], "It is generally assumed as self-evident, that the entropy of a gas is to be taken twice as large, if the number of molecules and the volume are both doubled. Now it is certainly true, that the *increase* of the entropy in a given process in a gas of twice the number of molecules is twice as large as the corresponding increase in the original gas. But what is the meaning of taking the *entropy itself* twice as large and *thereby settling the entropy-difference between the doubled and the original gas*? By what reversible process is the double quantity of gas to be generated from the original quantity? Without that the entropy difference  $\int \frac{dQ}{T}$  cannot be clearly defined".

Ehrenfest was, therefore, attacking Planck's concept of "absolute entropy", i.e. the use of Boltzmann's relation between entropy and thermodynamic probability to determine  $S$  itself through  $S = k \ln W$ , rather than to determine changes in  $S$  through  $S_2 - S_1 = k \ln (W_2/W_1)$ .

(It should be noted that the Ehrenfest–Trkal treatment does not involve the “absolute” value of the entropy since any *constant* factor in the  $Z$ ’s of Eq. (16) (i.e. any additive constants in the total entropy) would not affect the result in Eq. (19)).

PLANCK [35], of course, rose to the defence of the absolute entropy and responded to Ehrenfest’s criticisms<sup>1</sup>). Planck finally based his case for the  $(N!)^{-1}$  in the absolute entropy on the identity of the particles and the identity of two complexions which differ only by the interchange of two particles. Or, in other words, Planck insisted on the use of what GIBBS [36] called the generic rather than the specific phase. There is no record that Ehrenfest ever accepted this argument, and indeed it still does not make clear why the partition function should not be  $z^N$ , *if* the particles are *independent*.

(The Ehrenfest–Trkal paper had a direct influence on the important contribution to statistical mechanics made by Darwin and Fowler. It was this paper which suggested to Darwin “what a fruitful field statistics might be, and I tried applying to it certain mathematical tricks I had first used for other purposes. This started FOWLER and me on our work” [37]. In their papers and also in Fowler’s treatise the Ehrenfest–Trkal paper is spoken of most highly. Thus FOWLER writes (after referring to this paper), “I do not know of any earlier discussion of dissociation or the kindred matters of chemical constants, “thermodynamic probability” and absolute entropy ... which can be regarded as logically convincing” [38].)

We see that the same impasse which blocked the understanding of the statistics of photons was at least the cause of a detour in the statistics of atoms and molecules. In both cases the classical concept of the particle was at fault, since non-interacting classical particles are necessarily independent. Ehrenfest’s insistence on absolute clarity made him underline this point: the equivalence of the particles is already allowed for in the usual combinatorial analysis of statistical mechanics; something further is involved. That something, the lack of *independence* of the particles of matter and of radiation, emerged from the wave mechanics.

#### IV. *Quantum Statistics*

The theoretical developments in the years 1924 to 1927 created the quantum mechanics and the related quantum statistics in essentially the forms we use today. While Ehrenfest did not play a leading part in these developments, his critical insights as usual assisted greatly in the understanding of the flood of new ideas. He published only a few papers on

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<sup>1</sup>) Planck’s language is worth quoting here as an indication of his opinion of Ehrenfest. “If Mr. Ehrenfest expresses himself in the manner quoted, then one may unhesitatingly assume first that the question raised is worth a fundamental investigation, but also that the treatment it has hitherto received has not yet led to a satisfactory outcome”.



quantum statistics, in collaboration with his student Uhlenbeck, and it was Uhlenbeck who, in his thesis, systematized and unified much of the previous work in this field. In fact, as I shall indicate, Uhlenbeck's thesis showed how the statistics of Bose-Einstein and Fermi-Dirac answered many of the questions raised by Ehrenfest which have been discussed in the earlier sections of this paper. Before considering the papers of Ehrenfest and Uhlenbeck it is necessary once again to review the background for their work.

Bose's paper [39], published in 1924, was conceived as a new derivation of the Planck radiation law. Bose showed that the Planck law could be derived by treating the radiation as consisting of particles, photons, but that the possible states of a collection of such photons had to be counted by a procedure different from that normally used in the statistics of particles. In a sense, Bose's derivation was formally much like that given by Planck, and discussed above, except that Bose speaks of the radiation as composed of particles of energy whereas Planck "quantized" the energy of linear oscillators which absorbed or emitted the radiation. Bose's new statistical procedure was applied by EINSTEIN [40] to a gas composed of material particles, an ideal monatomic gas, and Einstein showed that, while at high temperatures such a gas behaves just as classical Boltzmann statistics predict, at low temperatures a new type of "condensation" occurs. Einstein also showed that the energy fluctuations in the gas obeyed an equation of the form of Eq. (11) with *both* terms present, whereas a Boltzmann gas would show only the first term, as has already been mentioned. EINSTEIN suggested the relationship of the second or wave term to de Broglie's theory of matter waves which had just been announced [41].

A year later FERMI [42] developed still another statistical procedure for a monatomic ideal gas, this one being appropriate when the Pauli exclusion principle applies to the particles of the gas. Fermi's work preceded the development of wave mechanics by Schroedinger; (he used oscillator energy levels for the particles, for convenience, treating the walls of the container as an idealized oscillator potential). It was DIRAC [43] who showed in 1926 how both the Bose and the Fermi statistics could be interpreted, from the point of view afforded by the wave mechanics, as the statistics obeyed by a collection of non-interacting identical particles whose total wave functions are required to be respectively symmetric and anti-symmetric with respect to permutations of the particles.

One of the important questions left unanswered by Dirac's work was simply: what about Boltzmann statistics? Does wave mechanics require Bose or Fermi counting procedures, or are there cases in which the classical Boltzmann counting is appropriate? This question was answered in a short paper by EHRENFEST and UHLENBECK [44]. They showed that the wave mechanics itself does not require one of the new statistics; it is only the imposition of specific symmetry requirements on the wave

functions which fixes the statistics, so to speak, and if no symmetry restrictions are used to select particular solutions of the wave equation then Boltzmann counting is natural and appropriate.

The argument of their paper is quite simple. They consider a one dimensional gas of  $N$  identical particles, each of mass  $m$ , in a container of length  $l = \pi$ . An eigenfunction of the Schroedinger equation has the form

$$(22) \quad \psi = \sin k_1 x_1 \sin k_2 x_2 \sin k_3 x_3 \dots \sin k_N x_N$$

where  $k_i$  are integers. The corresponding energy eigenvalue can be written as

$$(23) \quad E = \epsilon_{k_1} + \epsilon_{k_2} + \dots + \epsilon_{k_N} = \sum_s n_s \epsilon_s$$

where  $\epsilon_s = (\hbar^2/8\pi^2 m)s^2$  and  $n_s$  is the number of  $k$ 's having the integer value  $s$ . All the functions obtained from  $\psi$  by permuting the particles are also eigenfunctions having the same set of values  $k_i$  and the same energy  $E$ . The number  $G$  of such distinct eigenfunctions is  $N!/n_1!n_2!\dots$ . In particle language this  $G$  is described as the number of distinct ways in which the  $N$  particles can be divided into groups containing  $n_1, n_2, n_3 \dots$  particles, where the particles in group  $s$  each have energy  $\epsilon_s$ .

If, however, the wave function must be symmetrical, there is only one such function for a given set of  $k_i$ , the sum of all functions obtained from  $\psi$  by permutation of the particles. This is the Bose case. The requirement that the wave function must be anti-symmetric, the Fermi case, also restricts the possibilities to one function, the Slater determinant based on  $\psi$ . This determinant vanishes if any two of the  $k$ 's are the same, so that in this case the possible values of  $n_s$  are only 0 and 1, corresponding to the Pauli principle. Hence all three statistics are consistent with the wave mechanics and it is only the fact that one or the other symmetry requirement is imposed on actual particles which gives rise to the Bose and Fermi statistics for, say, photons and electrons.

There were other relationships between the new statistics and the old which had to be investigated, and this investigation was done systematically in UHLENBECK's thesis [45]. One of the important starting points of Uhlenbeck's work was a paper [46] by SCHROEDINGER on Bose-Einstein statistics, published early in 1926, which preceded his wave mechanics. Schroedinger began with the remark that the Einstein theory of the ideal gas seems to have as its essential point the application of a new kind of statistics, the Bose statistics, to the motion of the gas molecules, and that it goes against the grain to take this new statistics as something primary and incapable of being explained further. He went on to say that one would expect a deeper insight into the essence of this theory if one could develop it using the old well-established statistical method of Boltzmann, shifting the new assumptions to a place where they can be accepted without "sacrificium intellectus". The key to carrying out this program is the observation that although Bose statistics provide the particle

statistics needed for a particle interpretation of Planck's radiation law, this same law can be derived by applying the old "natural" statistics to the quantized harmonic oscillators which, in the usual wave picture, make up the degrees of freedom of the radiation field in an enclosure. Schroedinger then adapted this same change in viewpoint to the case of the material gas by applying "natural" Boltzmann statistics to the corresponding system of normal modes in the appropriate wave picture for the gas, the system of standing de Broglie waves. In this way all of the results of the Einstein gas theory were obtained without the apparently artificial Bose statistics.

Uhlenbeck made use of the new wave mechanics to extend Schroedinger's discussion of Bose statistics into a thorough treatment of the various ways in which Boltzmann, Bose and Fermi statistics can be interpreted. (This discussion appears in Section 8 and 9 of his fourth chapter, which is central for our discussion.) In the case of radiation, as Schroedinger had already pointed out, Bose statistics takes its natural place in the *wave* interpretation, where one is counting the distribution of energy over the various possible normal modes of the radiation. In a *particle* interpretation of radiation, however, Boltzmann statistics would be the only "natural" choice and it must be rejected for radiation since it leads to Wien's law and not Planck's law. The use of Bose or Fermi statistics in a *particle* picture is not "natural" because the particles involved are then *not independent* but have an ununderstood interaction which does not show up in the energy.

Similar remarks apply in the case of a (material) ideal gas: that is, the Boltzmann statistics are the "natural" choice in a particle picture and the Bose statistics are the "natural" choice in the wave picture. Here, however, one must distinguish between what Uhlenbeck calls the " $\mu$ -aether" and the " $\Gamma$ -aether". In the former case one describes the system in terms of the eigenstates of the single particle, whereas in the latter case, one uses the eigenstates of the whole gas considered as a single system. The remark above, about Bose statistics being "natural" in the wave picture, refers to the " $\mu$ -aether". In the " $\Gamma$ -aether", for the first time, the three statistics, Bose, Fermi, and Boltzmann, appear on the same footing according to whether one requires the total wave functions to be symmetric or antisymmetric with respect to permutations or one imposes no symmetry restrictions at all. (See the discussion above.)

To sum up this discussion, the critical point in the new statistics of particles is that they bring in the lack of *independence* of the particles in a natural way *in the appropriate wave interpretation*. As EHRENFEST put it [47], "One must distinguish between identity and independence, between the particle and the wave picture". After all, "*because the molecules are identical* Boltzmann put in the permutation factor  $N!/n_1! n_2! \dots$ ".

This lack of independence of the particles in an ideal quantum gas is also put in evidence in the fluctuation formula discussed above. Later



work has shown in great detail how this lack of independence manifests itself in an apparent attraction of Bose particles (or repulsion of Fermi particles) which contributes to the virial coefficients and leads to spatial correlations in the positions of the molecules.

Another point to be mentioned is related to the famous  $(N!)^{-1}$  which we have considered in Section III. It can be shown fairly directly [48] that for a Bose or Fermi gas at temperatures which are not too low the partition function has the form

$$(24) \quad Z = z^N / N!$$

where  $z$  is the "single-particle partition function", and  $N$  is the number of particles in the gas. This formula for  $Z$  differs from that for a classical gas, Eq. (20), by just the factor  $(N!)^{-1}$  needed to make the entropy an extensive function. This  $(N!)^{-1}$  comes essentially from the fact that where classically there were  $N!/n_1! n_2! \dots$  possible states there is only one quantum mechanically; in addition, the numbers  $n_s$  are between zero and one, on the average, in the temperature region under consideration, so that there are just  $(N!)^{-1}$  fewer states over which the partition sum is carried out.

Although the presence of this  $(N!)^{-1}$  is satisfying, because it makes the entropy extensive, it is not so significant as some would have it appear [49]. The reason, which was pointed out by Ehrenfest and Trkal in 1920 (see the discussion in Section III), is that only entropy differences are pinned down by the thermodynamic framework, and these suffice for all measurable effects including those involving the entropy constants. This point is underlined by Uhlenbeck when he says that nothing substantial would be changed if individual quantum states of a gas were given a weight other than unity; any constant weight would do, even a function of  $N$ , since  $N$  is fixed. Thus, by using an artificial (but permissible) weighting factor of  $N!$  for each state, one could bring  $Z$  back to the old form of Eq. (20). It is still true in quantum statistics, as it was classically, that the dependence of the thermodynamic functions on  $N$  remains conventional since  $N$  cannot be changed reversibly.

I should like to conclude this discussion of Ehrenfest's contributions to the ideas of quantum statistics with a brief treatment of one of his ideas which turned out to be wrong. Ehrenfest's passion for clarity and for the full understanding of new ideas led him to look for a direct physical interpretation of the Pauli exclusion principle. At the close of his paper [41] with Uhlenbeck he had written, "While the Pauli exclusion principle in its original form (prohibition of identical quantized orbits for the electrons in the atom) was so clear, there is up to now, so far as we know, no corpuscular interpretation of the extended exclusion principle (the requirement of antisymmetric wave functions)". This paper was sent to the *Zeitschrift für Physik* on December 15, 1926, but Ehrenfest did not put aside the problem of interpreting the Pauli principle. On December 29,

(while aboard the train from Leiden to Paris) he wrote to UHLENBECK [50] that he could interpret the exclusion principle as a consequence of the impenetrability of the electrons. The substance of this idea was communicated to *Naturwissenschaften* [51] and *Nature* [52] on January 9. The argument was simple: consider again a one dimensional gas of  $N$  particles. Impose the restriction of impenetrability: require that the wave function must vanish whenever  $x_h = x_k$ , for all  $h \neq k$ . The only linear combination of the  $N!$  degenerate eigenfunctions which will satisfy this condition is the completely anti-symmetric one. The apparent derivation of the exclusion principle from the requirement of impenetrability also makes it plausible that light quanta are not subject to the exclusion principle.

Unfortunately, however, Ehrenfest's reasoning was wrong. (This was first pointed out by FUES [53, 54]). His use of the simplest case, the one-dimensional gas "solely in order to keep the discussion brief" misled him, since it is not generally true that satisfaction of the impenetrability condition requires anti-symmetric wave functions. All wave functions are allowed, but the energies are altered slightly. EHRENFEST [53, 54] had to withdraw his assertion with the statement: "such a remarkable relation (as the exclusion principle) between the molecules of a gas cannot be explained by wave mechanics as simply as my mistake led me to believe".

One can nevertheless see in this failure the same qualities that made his great critical successes: the urge to connect the new ideas and the old, the need to see ideas absolutely clearly, the attention to fundamentals and not calculations, and the search for simple examples to illustrate the core of elaborate theories.

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# ASTRONOMY

## THE BASIC SCHEME OF ANY PLANETARY OR SATELLITE SYSTEM CORRECTED AND REANALYZED. I

BY

H. P. BERLAGE

(Communicated at the meeting of November 29, 1958)

### *Abstract*

The author gives a corrected version of the basic scheme of any planetary and satellite system, published in these Proceedings, 1957. He intends to prove that all known actual systems conform to this unified picture, thus supporting his theory of the origin of planets and satellites from discs of nebulous matter rotating round their primaries, by the mere mechanical transformation of these discs, through loss of energy due to viscosity, into sets of concentric rings.

The arrangement of the actual systems in the basic scheme and the relation between their Bode-ratio and the dimensions and mean densities of their primaries are surprisingly strict.

These rules suggest that a solar gaseous envelope still existed at the time when its dust content gathered into the rings which gave birth to the planets, whereas the planetary discs generating satellite rings consisted from the beginning of solid particles.

The author is led to the conclusion that Saturn and its system probably is the oldest condensation product of the solar nebula and that Pluto probably is a normal planet beyond Neptune in the terrestrial class, its mean density amounting to 2.9.

### *General remarks*

The basic scheme of any planetary or satellite system as developed in [10] needs revision. The author made an error in the calculations leading to the formulae (18) and (19). They should read

$$(1) \quad \begin{cases} \varepsilon e^{U_5} \cos (V_5 - p \ln a^2 r_m) = \frac{\psi}{a} 5! - \varphi 4! \\ \varepsilon e^{U_7} \cos (V_7 - p \ln a^2 r_m) = \frac{\psi}{a} 7! - \varphi 6! \\ \varepsilon e^{U_8} \cos (V_8 - p \ln a^2 r_m) = \frac{\psi}{a} 8! - \varphi 7! \end{cases}$$

and

$$(2) \quad \begin{cases} 210 e^{U_5} \cos (V_5 - p \ln a^2 r_m) - 21 e^{U_7} \cos (V_7 - p \ln a^2 r_m) + \\ + 2 e^{U_8} \cos (V_8 - p \ln a^2 r_m) = 0. \end{cases}$$

The constants  $\varphi$ ,  $\psi$  and  $\varepsilon$  are derived from the variation

$$(3) \quad \varrho_e = \varrho_0 (1 + \varphi) \exp \left[ -(a + \psi) r^{1/2} + \varepsilon \cos p \ln \frac{r}{r_m} \right]$$

applied to the relation

$$(4) \quad \varrho_e = \varrho_0 \exp [-ar^{1/2}].$$

The primitive equation (4) shows, as an effect of the first order, how the density of the gas or dust in the equatorial plane of a disc of gas or dust rotating round a massive central body,  $\varrho_e$ , depends on the distance from the axis of rotation,  $r$ , when viscosity has remodelled it so that it moves in the quasi-steady state corresponding to minimum loss of energy.

The expanded equation (3) shows, as an effect of the second order, through what state the disc approaches its final structure, a set of concentric rings whose radii obey a geometric progression.  $p$  is the frequency of the succession of circular condensations and rarefactions in the rotating disc. If  $b$  is the ratio of the distances from the centre of two successive satellites, or Bode-factor, then

$$(5) \quad p \ln b = 2\pi.$$

$r_m$  may be any distance from the centre where the density in the equatorial plane is known to pass through a maximum value.

Fig. 1 represents (2) in nomogrammatic form and is therewith the corrected form of the basic scheme of any planetary or satellite system. The curves representing the distances where condensation occurs in the primeval disc rotating round its primary are full drawn lines, the curves representing the distances where rarefaction occurs are broken lines. Any vertical cuts the full drawn curves in a series of points which may represent the location of a series of satellites circulating round a planet or of a series of planets circulating round their mother star.

Natural evolution is assumed to prefer verticals through points of intersection between the curves of condensation maxima and the horizontal

$$(6) \quad ar^{\frac{1}{2}} = 7 \text{ or } \ln a^2 r = 3.89$$

representing the distance from the centre where the unique satellite would grow, if the system were reduced to one member. With reference to mass relation (24) this is the same as saying that the satellite destined to become the heaviest member of its group should tend to grow inordinately at the cost of the other members.

No appreciable correction of the nomogram was required for values of  $p$  greater than 4. Even Mars and its satellites suffered only a slight displacement from  $p=5.9$  to  $p=6.2$ . A significant revision of the scheme, however, had to be made for values of  $p$  smaller than 4, and consequently in particular there, where Earth and Moon should be located.

The solar system as a whole was placed in its average empirical position  $p=10.8$  [8], although it was shown [10] that the giant planets and the terrestrial planets more precisely occupy two different standard positions,



viz.  $p=10.2$  and  $p=12.5$ . The author moved Uranus and its system from the position  $p=14.6$ , which was previously assigned to it [10], to the position  $p=16.3$  better suiting its known Bode-factor. He found himself moreover induced to let Saturn and its satellite system drop one place vertically down, for reasons which will be given later.

In [10] attention was already drawn to a relation which undeniably exists between the mean density of a planet and the Bode-factor of its satellite system. This mean density, designated by  $\bar{\rho}$ , has now also been

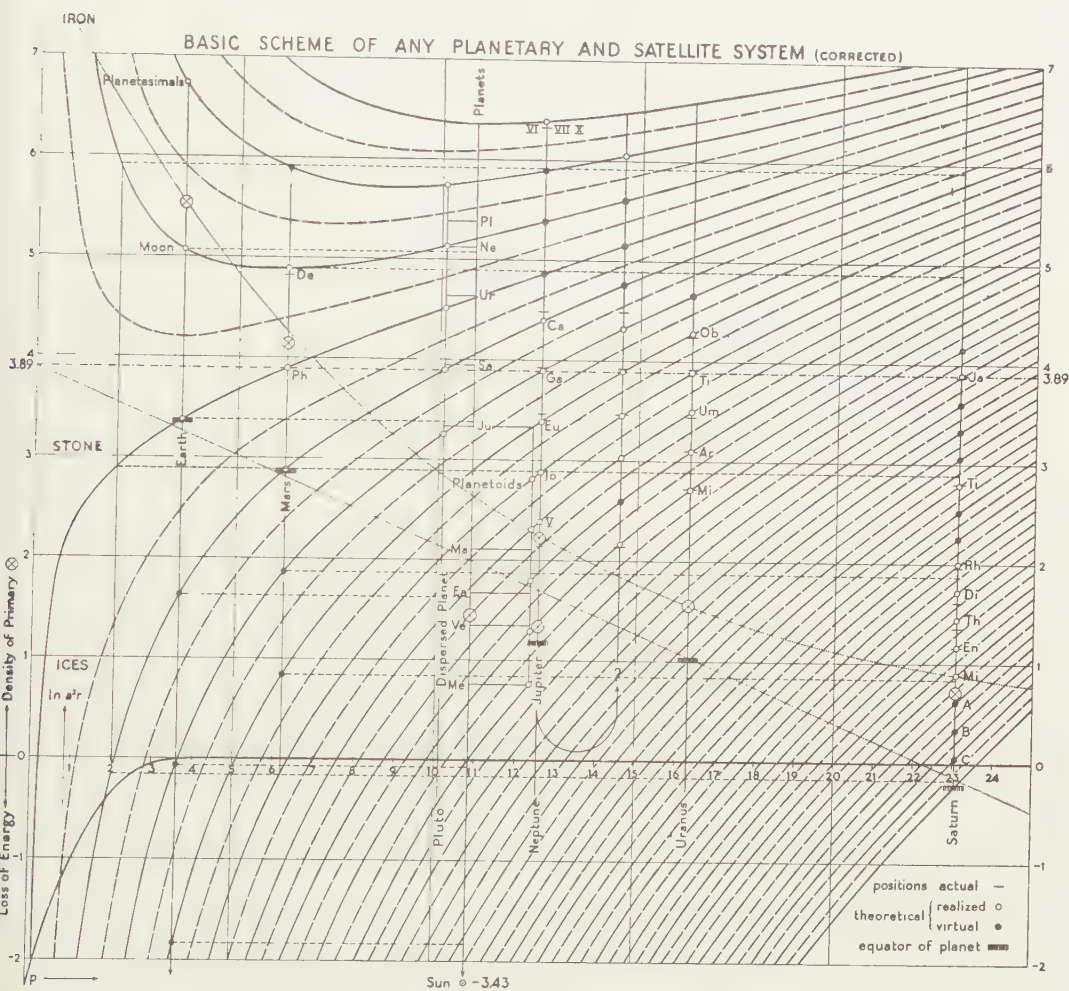


Fig. 1. The basic scheme of any planetary or satellite system corrected and reanalyzed

Earth (Moon). Mars (Ph=Phobos, De=Deimos). Planets (Me=Mercury, Ve=Venus, Ea=Earth, Planetoids, Ju=Jupiter, Sa=Saturn, Ur=Uranus, Ne=Neptune, Pl=Pluto). Jupiter (Io=Io, Eu=Europe, Ga=Ganymede, Ca=Callisto, V, VI, VII and X=other regular satellites). Uranus (Mi=Miranda, Ar=Ariel, Um=Umbriel, Ti=Titania, Ob=Oberon). Saturn (C, B, A=Rings, Mi=Mimas, En=Enceladus, Th=Thetys, Di=Dione, Rh=Rhea, Ti=Titan, Ja=Japetus).

plotted in the diagram and we are confronted with the approximate validity of the equation

$$(7) \quad p + 10 \ln \bar{\varrho} = 10 \ln \varrho_c$$

where  $\varrho_c$  is the mean density of the planet corresponding with  $p=0$ , apparently a certain critical density. The equation best adapted to the points given

$$(8) \quad p + 10 \ln \bar{\varrho} = 20.7$$

has been represented by the dotted curve. We conclude that

$$(9) \quad \varrho_c = 7.9$$

and note in passing that  $\varrho_c$  is the density of iron, a fact which can hardly be fortuitous. A remarkable consequence is that probably planets heavier than iron remain incapable of producing satellites, or else that discs consisting of iron meteorites are completely agglomerated to the primary by internal friction.

Three density levels have been distinguished and marked in the monogram, those of the iron meteorites, of the stony meteorites and of the cometary ices. Particles of these three kinds are the three specifically different solid planetary building materials, the planetesimals, whereas the gases hydrogen, helium (and neon) which are permanent within the confines of the solar system constitute the fourth kind (compare Fig. 2).

To simplify, round (8) off to

$$(10) \quad p + 10 \ln \bar{\varrho} = 20.$$

Now it should be noted that the points on the diagram concerning the Sun, Jupiter and Saturn decreasingly stay below the dotted curve. We can hardly be wrong in attributing this discrepancy to the hydrogen and helium content of these bodies which probably is 98, 87 and 69 per cent respectively. The latter two figures were derived by HARRISON BROWN [13]. The corresponding figures for Uranus and Neptune are 6 and 0.5 per cent respectively, if Neptune's density amounts to 2.22, the value derived by KUIPER in 1949 [17] and confirmed by CAMICHEL in 1953 [14]. The author hesitated at the high value 2.47 for the mean density of Neptune published by KUIPER in 1952 [18] and used here the first of these two values, placing Neptune accordingly in the position  $p=12.5$ .

Equation (10) is verified in Table 1

TABLE 1

	$p$	$10 \ln \bar{\varrho}$	$\Sigma$
Earth	3.6	17.1	20.7
Mars	6.2	14.2	20.4
Jupiter	14.6	3.0	17.6
Saturn	23.1	-3.4	19.7
Uranus	16.3	4.5	20.8
Neptune	12.5	8.1	20.6
Sun	10.8	3.4	(14.2)
		average	20

At this point the author must notify that he in fact gave in to his impression that not only Uranus but also Jupiter and its system had to be shifted one standard place towards the right, that is from its previously assumed position  $p = 12.5$  to the next position  $p = 14.6$ , although this removal is perhaps less strictly warranted and has been only suggested in the nomogram by an arrow. It includes the clear fixation of Jupiter V at the Bode-level which is not one but two steps below Io's level.

Besides relation (8) the nomogram demonstrates the validity of the approximately linear relation

$$(12) \quad \ln a^2 r_p = \ln a^2 r_c - 0.18 p$$

where  $r_p$  is the planet's radius, while, following (6)

$$(13) \quad \ln a^2 r_c = 3.89$$

if  $r_c$  is the critical distance from the centre where a unique satellite would originate if the planet should succeed in restricting itself to the generation of only one secondary [6]. This case corresponds with  $p = 0$ . In Fig. 1 equation (12) is represented by the dotted straight line.

Let us round off (12) to

$$(14) \quad p + 6 \ln a^2 r_p = 23.$$

Equation (14) is verified in Table 2

TABLE 2

	$p$	$6 \ln a^2 r_p$	$\Sigma$
Earth	3.6	20.4	24.0
Mars	6.2	16.8	23.0
Jupiter	14.6	7.2	21.8
Saturn	23.1	-1.5	21.6
Uranus	16.3	6.6	22.9
Neptune	12.5	—	—
Sun	10.8	-17.3	(-6.5)
		average	23

Jupiter and Saturn again remain decidedly on the lower side, while the Sun and the planetary system are this time quite in discordance with the planets and their satellite systems. We shall see presently what probably is the reason of this discrepancy. Neptune and its abortive system [9] does not allow the determination of its  $a$ .

From (10) and (14) we deduce

$$(15) \quad 6 \ln a^2 r_p - 10 \ln \bar{\varrho} = 3$$

an equation proving that  $a$  also is a function of the mean density and the radius of the planet. From (15) follows

$$(16) \quad r_p = \frac{1.65}{a^2} \bar{\varrho}^{3/2}.$$



We are reminded here of the pressure-density relationship in celestial bodies derived by KOTHARI [16], RAMSEY [23] and others,

$$(17) \quad P = 9.9 \times 10^6 (Z/A)^{1/2} \bar{\rho}^{3/2}$$

where  $A$  denotes the atomic weight and  $Z$  the atomic number of the element composing the body. As a matter of fact, we need hardly doubt how (16) must be interpreted physically. The greater  $r_p$  the greater  $\bar{\rho}$  by compression. The factor  $a$  increases with the density of the condensing matter in the fluid or solid but uncompressed state, and the question arises in how far (16) would be applicable to the Sun. Computing the "virtual" mean density of the Sun  $\bar{\rho}$  satisfying (15) with the aid of  $a = 6 \times 10^{-7} \text{ cm}^{-1/2}$  [8] and  $r_p = 6.95 \times 10^{10} \text{ cm}$ , we obtain

$$(18) \quad \bar{\rho} = 0.08.$$

This value agrees so remarkably well with the densities of hydrogen and helium in the fluid state, which are 0.07 and 0.15 respectively, that we are almost challenged to consider equation (15) as generally applicable.

Now,  $a$  is a factor which, while internal friction is doing its work in the primary's envelope, increases with time. In the case of the solar system evidently the differentiation of the original disc into rings was introduced at a moment mainly determined by the presence of hydrogen and helium, that is, by the fact that the disc was mainly gaseous.

The creation of the planets and their satellite discs was a second stage in the evolution of the whole system and the factor  $a$ , which in the case of the planets is more nearly determined by the nature of the condensing matter, perhaps even increases with time in such a general way as might allow us to locate the origin of a given system in the course of time according to its factor  $a$ . The three giant planets which are comparable from this point of view are Jupiter, Saturn and Uranus, whose factors  $a$  are [8, 10]  $2.2 \times 10^{-5}$ ,  $1.3 \times 10^{-5}$  and  $3.4 \times 10^{-5} \text{ cm}^{-1/2}$  respectively.

These values would therefore favour the view that, after the formation of the primary system, Saturn and its satellites were the first to be developed. Jupiter followed, then Uranus and then the other planets and their systems.

Now, final condensation will only occur where the density in the densest part of a ring outgrows the minimum value stated in Roche's theorem, that is when [5]

$$(19) \quad \rho_0 \exp [-ar^{1/2} + \varepsilon] > 14.5 M (\frac{4}{3}\pi r^3)^{-1}$$

where  $M$  is the mass of the primary, or when at least

$$(20) \quad \varepsilon = \ln 14.5 M (\frac{4}{3}\pi \rho_0)^{-1} - 3 \ln r + ar^{1/2}.$$

A maximum  $\varepsilon$  is attained where

$$(21) \quad \frac{d\varepsilon}{dr} = 0$$

and hence the distance from the centre where condensation is most likely to occur first is there, where

$$(22) \quad -3r^{-1} + \frac{1}{2}ar^{-1/2} = 0$$

that is where

$$(23) \quad ar^{1/2} = 6 \text{ or } \ln a^2r = 3.59.$$

Since Jupiter actually corresponds with  $\ln a^2r = 3.32$  and Saturn with  $\ln a^2r = 3.93$  we might favour, as I did in a previous paper [5], the conception that heavy Jupiter and its satellite system was the first of the systems to develop. There is however much to be said for the revision of this conception and for the assumption that Saturn started condensation before Jupiter. This may serve for a better understanding of Saturn's structure, its highly differentiated axial rotation and very low mean density, which makes this planet by far the most sunlike of the big four.

Next consider the factor  $p$ . In the satellite systems the factor  $p$  depends on the density and radius of the primary, meaning in these cases, most probably, that  $p$  depends on the kind and the mean density of the planetesimals which compose the system. The function of the radius of the primary in the conception of a system will be discussed later.

The remarkable fact, that the Bode-factor characterizing the solar system as a whole agrees numerically with an average of the Bode-factors characterizing the satellite systems, forces us almost to assume that this factor generally depends on the nature of the disc's material and through this nature only indirectly on the internal structure of the primary. This relation is rather well established in the case of satellites and planets, but almost entirely absent in the case of planets and Sun. This would include the proposition that planetesimals were the building material not only of the satellites, but also of the planets. Even the giant planets most probably were not originally the immense gaseous spheres consisting for the greater part of hydrogen and helium of the protoplanet type conceived by KUIPER, but may well have appropriated their hydrogen and helium by circulating for a long time after their formation inside the hydrogen and helium cloud which surrounded the primeval sun, but which withdrew by the process described by VON WEIZSÄCKER [27] and LÜST [21]. This process was perhaps promoted by the electromagnetic coupling which must have existed between the Sun and its rotating highly ionized gaseous envelope, foreseen by ALFVÉN [1] and recently reanalyzed by LÜST and SCHLÜTER [22] and BLOCK [11].

At this stage of our discussion we should note the remarkable fact that the basic scheme is not only the summary of all mechanically possible systems as which it was designed. As a matter of fact the actual systems within the solar system are arranged in the scheme according to their radial succession in the planetary system, first from the left to the right: Earth, Mars, Jupiter, Saturn, and further from the right to the left:

Saturn, Uranus, Neptune; this course through the scheme being evidently determined by the mean density of the planet which decreases from the Earth towards Saturn and increases again from Saturn to Neptune.

One interesting conclusion is that the "virtual" planet between Mars and Jupiter, which was dispersed and probably generated most of the comets, needs to be placed in the position  $p = 10.2$ . This dispersed planet, the total mass of which was earlier determined at 2.57 terrestrial masses [8], should have presented a mean density  $\bar{\rho}$  of 2.9. Following an earlier suggestion [7] the author is now inclined to favour the hypothesis that, at the distance from the Sun where the planetoids were formed, the planetesimals which accumulated into heavier bodies contained a large part of ice and that the boiling of the water by gravitational heating prohibited accumulations heavier than those of the satellite class.

A second consideration which arises now is that if Pluto is a true planet beyond Neptune, we should have to look for it in the same position  $p = 10.2$  corresponding with a mean density  $\bar{\rho} = 2.9$ . Pluto's mean density was assumed by KUIPER to be  $\bar{\rho} \sim 2$  leading through a value of 0.16 for Pluto's albedo to a value of 0.033 for Pluto's mass in terrestrial units [18]. Assuming at present the same albedo value, Pluto's mass might well prove to be at least equal to 0.05. This would place Pluto slightly below Mercury, but definitely in the terrestrial planets' range. An obvious advantage of this proposition is that Pluto acquires a much more suitable



Fig. 2. The mean densities of the bodies of the solar system in function of the logarithm of their masses.



place in the regular sequence of the densities of the bodies of the solar system in function of their masses, shown in [7], and reviewed in Fig. 2.

The two fundamental relations (10) and (14) prove that  $a$  and  $p$  both depend on the mean density of the planet  $\bar{\rho}$  and the radius of the planet  $r_p$ . The function of the planet's radius in the evolution of its satellite system, that is actually the function of its equatorial belt, is easily understood. The equatorial belt of the planet provides the inner boundary of the rotating disc, the development of which we are investigating. Viscosity transforms the disc and whereas near its centre the matter is naturally condensed on the planet's surface the accumulation towards the periphery is naturally stressed there where one unique satellite would grow. These two rings provide the basic "standing wave" naturally superimposed on the exponential radial density decrease (4) and between them, like the higher harmonics of a fundamental tone, one, two or more rings are interpolated, with extrapolations beyond (5) up to a certain ultimate distance from the centre proportional to the planet's mass. This is the outer limit where the potential of gravitation has dropped to that minimum value where the smallest of the planets possessing satellites, Mars, succeeded in creating Deimos [9].

It should be noted finally that

$$(24) \quad m_s = k m_p^{-1/2} r_s^{7/2} \exp [-a r_s^{1/2}]$$

is the mass of a satellite born at a distance  $r_s$  from the centre, where  $k$  is a function of  $\mu$  the weight of the particles composing the disc, molecules or meteorites,  $T$  the "temperature" of this matter,  $\rho_0$  the central density of the disc and  $p_0$  the  $p$ -factor which impressed upon the disc its dominant Bode-ratio. Hence in this case the greatest mass of a satellite is to be expected where

$$(25) \quad a r_s^{1/2} = 7 \text{ or } \ln a^2 r_s = 3.89$$

while generally

$$(26) \quad \log \frac{m_s}{m_p} = \log k + \frac{1}{2} \log \frac{r_s^7}{\bar{\rho}^3 r_p^9} - a r_s^{1/2}.$$

Equation (26) shows clearly why KUIPER [20] found an empirical relation of a simpler kind between  $\log m_s/m_p$  and  $\log r_s/r_r$ , where  $r_r$  is the planet's radius if its mean density is reduced to 1. As

$$(27) \quad \bar{\rho} r_p^3 = r_r^3$$

we obtain

$$(28) \quad \log \frac{m_s}{m_p} = \log k + \frac{9}{2} \log \frac{r_s}{r_r} - \log r_s - a r_s^{1/2}$$

the fundamental relation between the mass of a satellite and its distance from the planet in every system of the simple  $p_0$ -kind,  $k$  and  $a$  being two

constants differing from system to system. KUIPER is moreover puzzled by a "proximity effect" [20], a relative deficiency of mass of Jupiter's innermost satellite V (its outer neighbour is even lacking), and Saturn's innermost satellites Mimas and Enceladus. This relative deficiency of mass, however, quite simply derives from the fact that every transformation of a disc into a set of concentric rings may urgently require the superposition on the dominant  $p_0$ -wave of at least one secondary  $p$ -wave, say a  $p_1$ -wave longer than the  $p_0$ -wave and leading to the extended equation

$$(29) \quad \log \frac{m_s}{m_p} = \log k + \frac{9}{2} \log \frac{r_s}{r_p} - \log r_s - ar_s^{1/2} + \varepsilon \cos p_1 \ln \frac{r_s}{r_p}.$$

If

$$(30) \quad p_1 = \frac{1}{n} p_0$$

the most serious relative deficiency of mass of a satellite is to be expected at the nearest distance from the centre where

$$(31) \quad \cos p_1 \ln \frac{r_s}{r_p} = -1$$

or where

$$(32) \quad \frac{r_s}{r_p} = \exp \left[ \frac{n\pi}{p_0} \right]$$

The first satellite showing a relative surplus of mass then occurs where

$$(33) \quad \frac{r_s}{r_p} = \exp \left[ \frac{2n\pi}{p_0} \right].$$

Whereas on the planet's equator, that is at the distance  $r_p$  from the centre, the accumulation of mass reaches naturally a maximum, one or two satellites closely surrounding the planet will almost always show a more or less serious deficiency of mass relative to the satellites circulating at greater distance from the centre. That the light Martian pair, Phobos and Deimos, should also demonstrate the "proximity affect" is unjustly assumed by KUIPER [20]. If  $q_0$  is small  $k$  is small and then, of course, the masses of all the satellites of the system in evolution remain small. This is what evidently happened in the case of Mars.

*(To be continued)*

## ASTRONOMY

# THE BASIC SCHEME OF ANY PLANETARY OR SATELLITE SYSTEM CORRECTED AND REANALYZED. II

BY

H. P. BERLAGE

(Communicated at the meeting of November 29, 1958)

### *Abstract*

The author gives a direct proof of the tendency shown by secondaries to revolve round their primaries at distances which obey a geometric progression. It is concluded that every satellite system crosses the basic scheme for the right to the left and tries to realize one or more of the particular structures available to it, and characterized by increasing Bode ratio.

Our Moon probably is the remainder of a system of two significant satellites, the central one having been assimilated by the Earth. It crossed a light peripheral ring of planetesimals on its outward journey.

Phobos and Deimos probably are the remains of a system of three satellites, the central one having been assimilated by Mars.

The systems of Jupiter and Uranus, consisting of four significant members are the most normal results of satellite development.

Saturn's satellite system, although the most numerous known group of secondaries, proceeded with Titan in fact farthest on the way towards a one satellite formation.

The case of Neptune and its retrograde heavy satellite Triton is elucidated now that the position of this planet in the basic scheme could be fixed beyond doubt.

Planetoids and Plutoids are very similar bodies. The question is raised whether the tektites may originate from the Plutoids, as the stony and iron meteorites may originate from the Planetoids.

### *Why satellite distances tend to follow the geometric progression*

Before commenting on each particular satellite system within the planetary system the author wishes to prove more directly than before [5], and in a way which has, as far as he can see, many points in common with the way in which the same problem was treated by O. J. SCHMIDT [24] why a disc of gas or dust in quasi-steady rotation round a centre of preponderant mass tends to develop into a set of concentric rings whose radii obey a geometric progression.

As was already shown by GERASIMOVICH [15] and adopted by BERLAGE in one of his first papers on the origin of the solar system [2] steady motion of the disc would require that

$$(34) \quad \frac{d}{dr} \left\{ \eta r^3 \frac{d\omega}{dr} \right\} = 0$$



$\eta$  denoting the coefficient of viscosity. Because the circular motion is very nearly Keplerian, so that

$$(35) \quad \omega \sim r^{-3/2}$$

or

$$(36) \quad \frac{d\omega}{dr} \sim r^{-5/2}$$

steady motion can only subsist when

$$(37) \quad \eta \sim r^{-1/2}.$$

Now, the constant of viscosity is

$$(38) \quad \eta = \frac{\mu \bar{u}}{4\pi \sigma^2}$$

where the particles are characterized by

$$\begin{aligned} \mu &= \text{weight} \\ \sigma &= \text{diameter} \\ \bar{u} &= \text{"thermal velocity"}, \end{aligned}$$

or mean velocity vectorially superimposed on the velocity of circular Kepler motion.

We assume that we may leave aside whether the particles are of molecular or of meteoritic size, as long as the particles may be considered as large molecules, although not perfectly elastic.

In order to facilitate the solution of the problem, let us discuss a highly simplified two dimensional model. Suppose the radii of two near orbits of particles circulating round a mass  $M$  to be  $r$  and  $r + \Delta r$ , and the Kepler velocity of a particle circulating at a distance  $r$  to be  $v$ . Now, let this particle acquire a velocity  $v + \Delta v$  such that its new orbit, now elliptic, touches the circle with radius  $r + \Delta r$  and reaches the velocity  $v'$  at this latter distance from the centre. Then, if  $\gamma$  denotes the constant of gravitation,

$$(39) \quad v' \left(1 + \frac{\Delta r}{4r}\right) = \left(\frac{\gamma M}{r + \Delta r}\right)^{1/2}$$

while the Kepler velocity in the outer orbit is

$$(40) \quad v'' = \left(\frac{\gamma M}{r + \Delta r}\right)^{1/2}$$

Hence

$$(41) \quad u = v'' - v' = - \frac{(\gamma M)^{1/2} \Delta r}{4r^{3/2}}$$

in first approximation, while, at uniform global distribution of  $\bar{n}$

$$(42) \quad u = \frac{\pi^2}{16} \bar{u}$$

and thus

$$(43) \quad \eta = \frac{\mu}{4\pi \sigma^2} \cdot \frac{(\gamma M)^{1/2}}{4r^{1/2}} \cdot \frac{\Delta r}{r} \cdot \frac{16}{\pi^2}.$$

If the disc is of uniform composition  $\mu$  and  $\sigma$  are constants, so that

$$(44) \quad \eta = \text{constant} \cdot r^{-1/2} \frac{\Delta r}{r}$$

(37) and (44) together show that steady motion of the disc would require

$$(45) \quad \frac{\Delta r}{r} = \text{constant}.$$

Since the disc is known to show a tendency towards minimum dissipation of energy by viscosity, and thus will try to approach the ultimate goal of zero dissipation by its metamorphosis into a set of concentric rings, equation (45) certainly is an indication that the disc prefers a rearrangement of its matter into a set of concentric rings in Bode-succession.

An interesting point is that the Bode-ratio

$$(46) \quad b = 1 + 4 \frac{u}{v} = 1 + \frac{16}{\pi^2} \frac{\bar{u}}{v}$$

Hence the Bode-factor tends to increase with the ratio between the "thermal velocity" and the "Kepler velocity". Consequently it increases with the internal friction within the mass, the extreme case leading to the accumulation of all the matter on the planet, the Bode-factor becoming infinite, our  $p$ -factor becoming zero.

### *Earth and Moon*

As regards the Earth, characterized by only one satellite, the actual development of this system must have proceeded through the development of at least one other satellite of significant mass. In a previous publication [9] this second satellite was assumed to have circulated beyond our Moon in its early stage and to have been perturbed by the Moon and eventually thrown on to the Earth, creating our puzzling Pacific basin, a hypothesis which by its stress on a merely accidental catastrophe leaves much to be desired. According to the better picture which I wish to support here, the one mass gravitating towards the centre by loss of energy due to viscosity and providing the momentum for the accumulation of the second mass, was condensed on the Earth in its equatorial belt, while our Moon should be in fact that second outer satellite.

The curve delineating the boundary separating the two masses forming two rings is the broken line in the nomogram passing through a minimum at or very near  $p=3.6$ , the position of the Earth's system. This position means that in the case here considered the second satellite has the largest possible mass with respect to the first satellite. Very probably an evolution of this kind is facilitated, because it requires relatively less energy transformation than similar evolutions.

The Moon should have originated at a distance of 5.8 terrestrial radii and a "third" satellite at a distance of 5.8<sup>2</sup> or 33.7 terrestrial radii. This is quite in accordance with our previous conclusion [9] that the Earth's

system in gravitational conformity with the Martian system should have been able to produce a peripheral satellite at a distance of, but no more than, 35 terrestrial radii.

The annular embryo of this latter satellite undoubtedly never condensed into one body, but only into a large number of planetesimals. These planetesimals are likely to have bombarded the Moon, creating its "maria" and meteorite craters, when the Moon, veered out by tidal friction, passed the distance of 34 times the Earth's radius. Hence, the origin of these lunar features was probably rightly depicted by KUIPER [20].

The ratio of the mass of the Moon  $m_m$  and of the total mass  $m_e$  of the material of the disc circulating between the Earth's equator and the distance of separation given by  $\ln a^2 r_d = 4.17$ , is as

$$(47) \quad \frac{m_m}{m_e} = 0.76$$

whereas the ratio of the total mass of the planetesimals  $m_p$  and of the mass of the Moon is

$$(48) \quad \frac{m_p}{m_m} = 0.00058$$

These are numerically interesting conclusions. UREY [26] estimated the diameter of the planetesimal, the largest of all, which generated the Mare Imbrium, at 230 km. The diameter of the Moon is 3476 km. If the density of this planetesimal is assumed to be equal to the density of the Moon, the ratio of the masses of the two bodies is

$$(49) \quad \frac{\text{Mare Imbrium Planetesimal}}{\text{Moon}} = 0.00048.$$

This particular mass is in excellent agreement with the total mass of planetesimals given by (48).

We will terminate the discussion of the terrestrial system by computing the duration of revolution of the Moon at its birth. It was 19.4 hours. In these conditions the ratio of the angular momenta of Moon and Earth, which at the present time is roughly 4 : 1, was roughly 1 : 3. The day counted roughly 6.5 hours.

Now, a satellite of 1.3 times the Moon's mass would count for 3.1 times the Moon's angular momentum when accumulated in the Earth's equatorial belt. As just this amount of angular momentum must have been conferred on the Earth good agreement is achieved, if only we are allowed to assume that, practically, the Earth did not rotate after its own formation. This, however, may well be considered as a logical consequence of the strong solar tidal friction which must have operated during the time when the primeval Earth was sufficiently fluid to permit the concentration of its iron-nickel nucleus.

The position assigned to the Earth and its system in the scheme is no longer directed by the horizontal axis indicating the distance where one satellite would grow, whereas the structure of the Martian system leaves



no doubt about the appropriateness of this axis. The reason for this, however, is that according to our present view the Earth's system, seemingly a one satellite system is to be considered genetically as a two satellite system (the rest masses being negligibly small), whereas the Martian system, seemingly a two satellite system is to be considered genetically as a three satellite system, the inner satellites in both cases having been received on to the planet's surface.

### *Mars and its satellites*

The position of Mars and its system in the scheme allows the computation of the ratio of the masses of the two satellites Phobos and Deimos. It is 0.41 if no surplus mass was delivered to Phobos on account of its position on the 3.89 horizontal axis through the scheme. Since the ratio of these masses is probably more nearly equal to the ratio of their volumina, that is the ratio of the third powers of their diameters, usually estimated at 15 and 8 km, or equal to 0.15, there are serious arguments for this stress on the mass of Phobos having actually occurred, as apparently it did in other cases, for instance, during the formation of Ganymede and Titania in the systems of Jupiter and Uranus.

One point should be elucidated here. If Phobos, although a very small body, has indeed managed to create such tidal deformation of Mars that this satellite, whose angular velocity is much greater than the planet's angular velocity, has approached the planet since its birth in a significant manner, as was recently suggested by SHARPLESS [25] and adopted by KUIPER [20], then we place it wrongly in our scheme. The author, however, is inclined to wait for decisive confirmation of this suggestion.

### *Jupiter and its satellites*

The logarithm of the ratio of the masses of Jupiter's satellites, as far as circulating in the direct sense, and the planet's mass are

V	— 8.75
I=Io	— 4.42
II=Europe	— 4.60
III=Ganymede	— 4.09
IV=Callisto	— 4.39
VI, VII, X	very small

Obviously the mass of Jupiter V is too small, Io's mass too great and perhaps Europe's mass again too small for its place in the series. This secondary fluctuation superimposed on the normal trend of the masses through a simple maximum near the distance defined by (6) is readily explained by the tendency of Jupiter's disc to evolve through its dominant  $p_0$ -value some way towards a smaller  $p_1$ -value. The fine-structure of Jupiter's system is, however, insufficiently developed to permit an undubious specification of this  $p_1$ -value.

*Saturn and its satellites*

I refer here to my earlier analysis of this system as regards masses [8]. Saturn's system was placed in the scheme in the position  $p = 23.1$ . I noted, however, that the factor  $a$  was not sufficiently assured to exclude small corrections in this position if these should be required. The system was dropped, as already pointed out, one place down, therewith reaching better agreement with the Martian system in the succession of satellites. We find a replica of the Martian Bode-ratio in

Saturn's equator  
Mimas  
Dione  
Titan  
Japetus

explaining at the same time, in accordance with the present theory, that secondary stress was laid on the masses just listed.

It is true that Dione is not in reality distinguished by relatively large mass. This satellite however does appear (see Fig. 2) distinguished by abnormally high density, suggesting that by some unknown cause its mass was greatly reduced by the loss of a large part of its ice content.

A very serious question, however, has always been how Titan obtained so large a mass that the masses of all other satellites are very small besides it, since, as we know, a planet is scarcely able to produce only one secondary. The solution of this curious fact has now become apparent. The scheme discloses the possibility of creating on the basis of the planet's equator a satellite system with  $p \sim 2$  providing maximum density and hence a heavy satellite at exactly Titan's distance from Saturn. The next density maximum of this kind follows far beyond the system's natural periphery.

In this way the three prominent frequencies of the density fluctuations which we recognize in Saturn's satellite system are very near

$$p = 24, 6, 2.$$

These values are to be compared with the empirical ones, given in [8], viz.

$$p_0 = 23.1$$

$$p_1 = 5.2$$

$$p_2 = 1.9.$$

*Uranus and its satellites*

The logarithm of the ratio of the masses of the five known satellites of Uranus and the planet's mass are roughly

Miranda     $-6.0$   
Ariel        $-4.8$   
Umbriel     $-5.2$   
Titania      $-4.3$   
Oberon      $-4.5$ .

While in Jupiter's case it is Io's mass, in Uranus' case it is Ariel's mass that is evidently too great for its place in the series, if referred to the general trend.

The system of Uranus is an almost perfect copy of the system of Jupiter and whereas contrary to the mass of Jupiter V the mass of Miranda is not too small for its place, we may even say that Uranus' system is the most normal one of all. It shows the slightest tendency of all known systems to adapt itself to other  $p$ -values than its dominant  $p_0$ -value.

This conclusion stresses the probability, mentioned already in [9] that one or two small satellites of Uranus beyond Oberon await discovery.

### *Neptune and its satellites*

As to this system whose place in the scheme now became evident, we refer to the hypothesis on its origin, which was considered by KUIPER [19] and BERLAGE [9], and to the present arguments in favour of the conception that Pluto is a normal independent planet.

Equation (14) and its graphical representation yield

$$(50) \quad \ln a^2 r_p = 1.70$$

for the level of Neptune's radius in the scheme. Assuming this value to be reliable we would encounter the retrograde Triton, which circulates at a distance  $r_t = 13.3 r_p$  at the level in the scheme given by

$$(52) \quad \ln a^2 r_t = 4.29.$$

If the whole mass of the disc were united in one satellite at a distance  $r_c$  from the centre, then, according to (6)

$$\ln a^2 r_c = 3.89.$$

Hence

$$(53) \quad \frac{r_c}{r_t} = 0.67$$

If  $m_x$  denotes Triton's mass and  $r_x$  Triton's distance from the centre when this satellite started its retrograde revolution round Neptune, and if  $m_t$  denotes Triton's present mass and  $m_d$  the total mass of the directly rotating disc which was consumed by Triton, the constancy of the total angular momentum of the system requires that

$$(52) \quad m_x r_x^{1/2} - m_d r_c^{1/2} = m_t r_t^{1/2}$$

while

$$(53) \quad m_t = m_x + m_d.$$

Hence

$$(54) \quad m_x \left\{ \left( \frac{r_x}{r_t} \right)^{1/2} - 1 \right\} = m_d \left\{ \left( \frac{r_c}{r_t} \right)^{1/2} + 1 \right\}.$$



Inserting (53) we obtain

$$(55) \quad \left\{ \left( \frac{r_x}{r_t} \right)^{1/2} - 1 \right\} = 1.82 \frac{m_d}{m_x}$$

If, for instance,

$$(56) \quad m_x = m_d$$

that is when Triton's mass was doubled, then

$$(57) \quad \frac{r_x}{r_t} = 2.82^2 = 7.95$$

and

$$(58) \quad r_x = 106 \, r_p.$$

As was already stated in [9] the assumption, that the retrograde Triton, originating as an independent byproduct of the extraordinarily broad ring which conglomerated into the outermost of the giant planets, was captured in Neptune's directly rotating disc and spiralled inward from a distance about 100 times the planet's radius, is quite reasonable. Triton crossed probably the orbit of Nereid, while this latter circulated in the direct sense at a distance of about 60 times the planet's radius. During this encounter Triton may have thrown Nereid into its actual highly excentric orbit in which it approaches the planet down to 55 times the planet's radius. Thus Nereid must have been already condensed at that time. The rest of the disc however remained apparently in the state of dust until it was completely absorbed by Triton.

Imagining Neptune's system as it might have developed regularly from Neptune's disc, our picture contains a series of satellites circulating in the direct sense at distances from the centre which are, when expressed in the planet's radius as a unit, following our scheme

I	II	III	IV	V	VI	VII	VIII
3	5	9	15	24	40	65	107

Neptune's big four should have been satellites II, III, IV and V, with the significant surplus of mass, due to be received by the satellite generated at the distance where a unique satellite might have grown, on satellite III. The smaller satellites I and VI aparently no more came into existence than did the big four. In VII, however, as follows from its distance from the primary, we may recognize Nereid, whereas VIII may have closed the system as, for instance, Jupiter's satellites VI, VII and X close Jupiter's system as far as concerns the direct satellites. Jupiter's retrograde satellites VIII, IX, XI and XII are a couple of extremely small bodies, yet, as also Saturn's Phoebe, genetically similar to Neptune's big Triton. Perhaps Triton's collision with Neptune's satellite VIII, while this one circulated in the direct sense at a distance of 107 times the planet's radius induced Triton's inclusion in Neptune's system.

### *Pluto or Plutoids*

It is a remarkable fact that we find Pluto, if indeed it is an independent planet, with the Planetoids in the same vertical in the scheme; that is the one corresponding with  $\bar{q} = 2.9$ . There are thus arguments in favour of the hypothesis, already earlier discussed, that Pluto is only the biggest or the nearest of perhaps quite a few Plutoids, and the author wonders whether D. BROUWER [12], who mentions that the cause of the puzzling deviation of Uranus in latitude "may well be of an exciting nature", is pointing in the same direction.

So the question arises whether we should trace along the periphery of the solar system the source of the tektites, meteorites composed mainly of  $\text{SiO}_2$  and containing only small percentages of iron, the element which evidently was already strongly concentrated near the Sun when planets formed.

### *Concluding remarks*

Our discussion has now led us to assume that Saturn's satellite system is the oldest among the satellite systems. Moreover the surplus energy proportional to

$$(59) \quad e^U \cos (V_5 - p \ln a^2 r_m)$$

which must be dissipated during the disc's transformation in order that the disc be settled in its final state of concentric rings, increases with decreasing  $p$ . It is therefore probable that every disc's development progresses through the scheme from the right towards the left, and not from the left towards the right as was previously assumed. The  $a$ -factor increases in the course of time, while the  $p$ -factor decreases. At first a shortwave circular ridging is spontaneously superimposed on the disc, and the embryonic rings successively grow together into more massive rings further apart, and this not unsystematically, but very systematically, leading to the differentiation of the actual systems. Every system has been sliding down an energy scale and falling from one potential energy pit into the next, the bottom of which is deeper, and finally stuck in the trap from which there is no return, the rings having separated for ever. Thus the end-trap may be differentiated into a few sub-traps characterized by different  $p$ -values, which finally determine the masses of all the members of the system.

In the planetary system as a whole, we find realized the succession

$$\begin{aligned} p_0 &= 10.8 \\ p_1 &= 3.6 \\ p_2 &= 0. \end{aligned}$$

In earlier publications [9] it was assumed that  $p_2 = 1.4$  contributed to the planetary system's structure, yielding a density maximum at  $\ln a^2 r = 4.2$ ,

that is between Saturn and Uranus. Such an arrangement does not find theoretical confirmation. The author's impression however is that the high density maximum marking the giant planets is already adequately taken care of by the density maximum at  $\ln a^2r = 3.89$  at Saturn's distance, corresponding with  $p = 0$ , towards which the development points naturally; the more so because we may, as we now know, be induced to count as the effective masses of the giant planets in this early stage of the evolution of the solar system the masses which these planets reach without their hydrogen and helium content. This would mean the following effective masses,

Jupiter	41.2	Uranus	13.8
Saturn	29.5	Neptune	17.2

when expressed in terrestrial units.

A potential system continuously losing energy thus wanders from the right to the left through those possibilities which are realized in the systems of Saturn, Uranus, Jupiter, and in the solar system itself. It may fall next into the Martian trap (as Saturn did), and further into the Moon-trap (as the solar system did), and lastly into the Titan-trap (as again Saturn did). This last trap leads the system in a certain sense further on the way towards a one-satellite system. So as we now see it this happened in the Saturn system by means of a successive reduction of its  $p$ -value from 24 through 6 to 2.

It is remarkable to state how quickly the amount of energy which must be dissipated during the development of a disc increases with decreasing  $p$  as soon as the Earth and Moon stage is passed. It is shown by the "loss of energy" curve in the lower part of the diagram, representing, in arbitrary measure,  $-0.1$  of the quantity defined by (59). Further operation in the same direction towards greater Bode-ratios is evidently no longer a second order process. It becomes more and more improbable and the author, although opposed against every hypothesis ad hoc not strictly required, is ready to admit that the Earth's system may have come into existence regularly only because the Moon's mass and the mass of its inner colleague were sufficiently great to make up for the total loss of energy of the system due to viscosity by the liberation of gravitational energy due to their proper condensation.

Looking at the matter from this point of view, it is worth while to speculate on the potential generation of satellites by Mercury and Venus. The undeniable succession of the planets in our scheme, first from the left to the right and then from the right to the left induces us to adjudge them positions near  $p=1$  and  $p=2$ .

Now, in view of this, our first remark is that the mean densities of Mercury and Venus,  $\bar{\rho} = 5.46$  and  $5.06$  respectively, would not suit (7). They remain below expectancy in the proportions  $5.46 : 7.3 = 0.75$  and  $5.06 : 6.5 = 0.78$ . The reason for this density deficiency of Venus and

Mercury may for instance be that the Robertson-Poynting effect is to select small iron grains from larger stone particles and draw them away towards the Sun in larger numbers from the rings which were to form Venus and Mercury than from the ring which was to form the Earth, during the time preceding the final condensation of these planets.

Our second remark is that these positions are too far too the left in the scheme to allow the generation of satellites. Discs of dust rotating round Mercury and Venus, if ever they existed, have most probably simply spiralled inward towards their centres and united completely with their planets. No indication of this unison remained in the axial rotation of Mercury, solar tidal friction prohibiting it completely. However, the fall of dust on Venus after solar tidal friction had finished its work apparently succeeded in producing a slight amount of axial rotation of this planet.

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## PHYSICS

# RADIOCARBON DATING OF THE FOSSILE SOILS AT OBER FELLABRUNN

BY

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(Communicated at the meeting of December 20, 1958)

### *Summary*

Samples from the well-known loess profile at Ober Fellabrunn (Austria) have been studied. Humus has been extracted from the fossile soils and the overlying loess; from 800 to 1500 grams of material was used.

Though a rough estimate demonstrates that the activities found could be due to recent contamination, the small amount of radiocarbon in the overlying loess proves that contamination (at least in this profile) is at most an unimportant correction. The ages obtained from top to bottom are: 16000 (years before present) for a slightly stratified loess with less than 0.03 per cent of humus; 32000 for a loess just overlying the upper black earth (humus-content 0.4 per cent); 38000 for the upper black earth (humus 1 per cent) and 42000 for the lower black earth (humus 0.8 per cent). The amount of carbon in the underlying weathering horizon was too small (less than 0.01 per cent) to obtain a date. The results are compatible with radiocarbon dates from other areas.

### *Introduction*

The climatic sequence and its chronology over the last 70 000 years in North Western Europe are fairly well established at this moment (see ref. [1]). The most challenging problem is now the correlation with the loess chronology and (or) the correlation with the subdivision of the Würm in Würm I, II and III. This problem will be discussed in more detail later in this article, since definite conclusions can only be obtained from radiocarbon dates of standard loess profiles. Unfortunately, no charcoal has been found in these sites. It was therefore decided to use the humus in the loess and fossile soils for this purpose.

### *The samples*

The samples studied here were carefully collected by Prof. Dr. J. Fink and Dr. F. Felgenhauer together with other members of the "Urgeschichtliche Arbeitsgemeinschaft", Vienna, in September 1958 at the well-known brick yard at Ober Fellabrunn (not far from Hollabrunn, 70 km north west of Vienna). The stratigraphy is shown in fig. 1. Further details can be found in ref. [2-4]. Each circle in fig. 1 represents one sample. In order to get uncontaminated material about 1 meter of the vertical profile was

removed before the present samples were taken. The samples were shipped in large tins to Groningen, each containing 10 kg of loess.

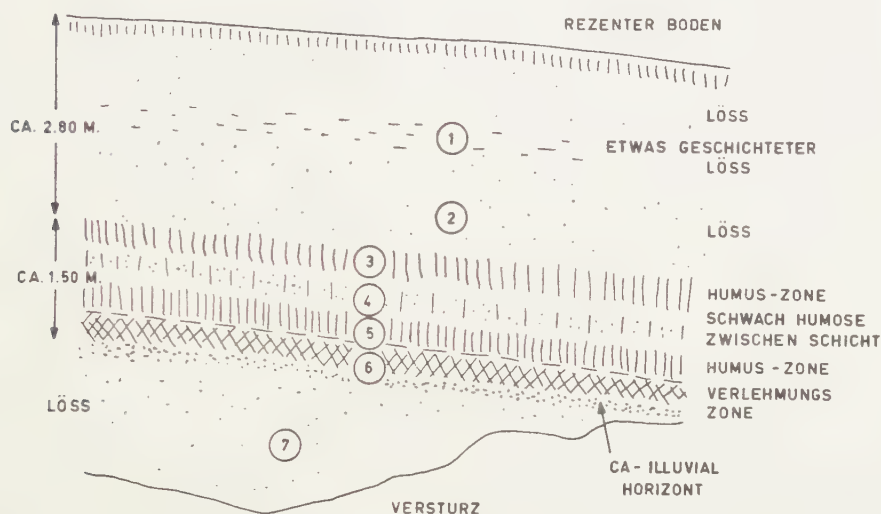


Fig. 1. South-looking side of the abandoned brickyard at Ober Fellabrunn (Sketch by Prof. Dr. J. Fink, 6/IX/1958). The drawing gives a cut somewhat left of the middle. Numbers in circles indicate the position of the samples.

### *Chemical preparation*

Because of the low content of organic carbon it was decided to extract humus from the loess and soils. This also secures that humus is dated and not recent rootlets, though some "humus" will also be extracted from roots. Ions like  $\text{Ca}^{++}$ ,  $\text{Fe}^{++}$  and  $\text{Fe}^{+++}$  have to be removed by thorough acid washing, before the humus can be extracted by alkali. Since the value of the total humus content is of importance for the discussion of the effect of recent contamination, the efficiency of the extraction should be high, or at least the same for all samples. Therefore all samples were treated in practically the same way. To begin with, concentrated HCl was added to dissolve carbonates. The concentration was 20, sometimes even 40 per cent; the concentration of  $\text{H}^+$  ions was never large, however, because of the large amounts of carbonates in the samples. The acid was added in steps until the  $p_{\text{H}}$  remained below 2.

Assuming that the acid was used for dissolving calciumcarbonate, the amount of this carbonate could be calculated (see table I). The percentage in the weathering horizon proper (sample 6) may have been somewhat lower since the sample contained some carbonate concretions.

The fluid, containing  $\text{CaCl}_2$  in a high concentration, was centrifuged off. Then the loess was stirred and rubbed again to a sludge. This is sometimes very laborious, especially if larger centrifugal forces are used. With 2000 g it is often difficult, with 400 g much easier, whereas the amount of fluid left in the sample is still fairly small.

Acid was added to the sludge (1 per cent HCl, 1.5 or 2 liter per kilogram loess) to keep the sample acid enough for extraction of iron (and perhaps other ions). These ions come easier off at a higher temperature; therefore the samples were put now in an oven at about 90° C for a few hours. Afterward centrifuged off again, etc. All together the samples were centrifuged seven or eight times. By adding alkali to the fluid it was checked that Ca<sup>++</sup> ions were well out after about four runs. From there on no acid but distilled water was added to the sludge. Iron was still present after the fourth run, especially in the case of unweathered loess. It consisted mainly of ferrous ions, and perhaps some other ion which gave the alkaline precipitate a more bluish colour. After the seven runs the fluid gave hardly any precipitate with alkali, but sometimes it still tasted very bad.

Then alkali was added (about 1.5 liter per kilogram loess, containing 1 per cent by weight of NaOH). This solution was not heated in order to avoid the extraction of humus from recent sources (roots, etc.) which might be more soluble in the hot alkali. The "old" humus came already off in the cold; consequently there was no reason for a more rigorous treatment. After about 12 or 24 hours the sample was centrifuged off, and the humus precipitated by acid. New alkali was added to the remaining loess and the extraction repeated. This gave the "second fraction". For some samples the extraction was repeated once more, but now with heating over night at about 90° C. It is shown in table I that there is no indication that this heating produces any extra extraction of humus. Though this third fraction was combusted for measurement of the carbon content, it was not put into the counter.

The humus could also be precipitated from the alkaline solution by CaCl<sub>2</sub>; this proves that removal of metal ions is essential for the extraction of the humus. The humus has to be precipitated by acid anyhow in order to avoid the absorption of CO<sub>2</sub> from the air. The acidified material is

TABLE I

Summary of results. Initial weight 850 grams; for (1) and (6) it was 1500 grams, but the data given here are per 850 grams. Column 2: content of calcium carbonate. The figure for sample 6 includes the carbonate in some carbonate concretions. Column 3-5: amount of carbon in fraction 1-3 (grams). Column 6: total activity in counts per minute of carbon extracted from 850 grams of the sample, uncorrected for the efficiency of the counter. Column: 7 our station number (Gro . . .). The ages given in column 8 may be too low because of recent infiltration (see text)

Sample	CaCO <sub>3</sub> %	Carbon			Total activity	Gro	Age
		1	2	3			
1	18	0.11	0.04	0.01	0.17 ± 0.02	1911	16500 ± 800
2	15	1.3	0.35	0.1	0.26 ± 0.02	1901	31600 ± 500
3	8.5	3.3	1.3	—	0.39 ± 0.05	1745	37600 ± 700
5	7.5	3.1	0.6	0.15	0.18 ± 0.02	1740	41900 ± 800
6	4.5	0.02	—	—	—	—	—

heated for a few hours, centrifuged, water added, sometimes plus some  $\text{CaCl}_2$  if the humus tends to go into colloidal solution, etc. until the  $\text{pH}$  is above 3. Then the humus is dried. It often contains inorganic material, some fractions up to 70 per cent. Part of it could have been removed by centrifuging the alkaline solution for another half hour, but since it did not interfere with the combustion no further efforts were made to remove the inorganic material. For removal of the last loess, and for the humus precipitate, always the larger centrifugal force (2000 g) was used. Even then sometimes part of the loess is difficult to remove since it goes into some colloidal solution. Instead of alkali sometimes a mixture of 0.1 N NaOH and 0.2 N NaF is recommended for extraction of the humus since it is claimed to prevent the loess from going into solution. Our experience is that a higher concentration of alkali is also effective.

The small amounts obtained from (1) and (6) are not due to an inefficient procedure. This was checked by repeating the acid treatment and the alkaline extraction completely on part of the residual loess. This second run did not produce any measurable amount of humus. The first treatment of (1) and (6) was actually somewhat different from the treatment of the other samples in as far as a larger centrifuge with four 2-liter stainless steel beakers was used instead of the smaller centrifuge with four 200  $\text{cm}^3$  glass beakers. It was checked that the stainless steel was not attacked by the acid samples if they were cooled first to room temperature. The large centrifuge had to be used in order to handle the larger amounts of sample (1) and (6) (1.5 kilogram) in a reasonable time.

Though even a third run on (1) and (6) did not produce any extra humus, ignition of the remaining loess gave nearly the same amount of  $\text{CO}_2$  as was obtained from the humus. Obviously the loess contains organic products not soluble in alkali.

### *Rootlets*

All samples studied, except (6), contained rootlets. For sample (2) and (5) they were picked out as far as they happened to swim on the surface. For sample (1) and (3) it was tried to get a more precise indication of their amount. Sieving of the wet loess turned out to be not very effective. There were too many larger lumps of loess, cemented by carbonate, to make the sieving very effective. Even after the greater part of the carbonate was removed by acid it was not very efficient and it turned out to be not better than picking out the rootlets which came swimming at the surface of the fluid which was fairly heavy because of the large amount of  $\text{CaCl}_2$ . Sample (1) and (3) gave 9 and 10 mg of (dried) roots. We guess that the total amount may have been twice as large but not much more. Our impression is that (2) and (5) contained somewhat more roots. At any rate there was no marked decrease with depth. This may indicate that the roots intruded from the vertical wall since the time the brickyard



was abandoned; this would imply that the present amount is much larger than it was up to a few years ago.

### *Discussion of possible contamination*

In order to get an impression of possible contamination by recent carbon the amount of C 14 per standard amount of loess (here 850 grams) is given in column (6) of table I. It is probable that the amount of recent carbon will decrease from the surface downward. So the activity of sample (1) would set an upper limit to the amount of infiltrated carbon. Since the other samples have a higher activity (per 850 grams of loess) at least an important part of their activity is due to the original organic carbon. Unfortunately, the results cannot exclude that some contamination occurred. Perhaps the loess between sample (1) and (2) contains material with a still lower content of radiocarbon which would provide a lower upper limit for the infiltration. Going down from (1) to (2) the specific activity of the original humus will be lower since it is older. If, at the same time, the humus content does not rise, that lower layer would give a better control sample.

Though the foregoing discussion demonstrated that the recent contamination was fortunately small in the present profile, the amount could have been much higher, as will be shown now by another approach. First of all we are only interested in humus, or material which is extracted together with the humus. It is highly improbable that humus moves downward in the calcareous soil as such. It is more probable that the organic material comes down as rootlets, which decay into humus. According to the previous section our samples contained about 0.01 grams of carbon as roots. This is the carbon in the *recent* roots only, however. One may estimate that the roots decay in, say, 8 years. Then over one average life of radiocarbon (8000 years) 1000 times more will have come down. This is 10 grams. We have to account for an activity of about 0.2/min. This corresponds in our counter to 0.02 grams of recent carbon. Though it is not known how much humus will be left of the roots which contained 10 grams of carbon, 0.02 grams seems to be a very low estimate, and this would mean that all activity could easily be accounted for by the roots going down. However, it was mentioned in the previous section that the present amount of roots is probably much higher than it has been in the past. At any rate this source of contamination can only be excluded by taking control samples, as discussed in the first part of this section.

One other source of humus has to be considered. If loess is redeposited from an eroded fossil soil, the new layer will contain older carbon. Fortunately the effect of old carbon is much less than the effect of recent carbon. One per cent of recent carbon, added to a 37 000 years old sample will make it 5000 years younger, whereas 100 per cent of old, inactive, carbon are required to make it 5000 years older.

### *Discussion of the age measurements*

According to the previous section, the dates obtained are probably correct. It is of interest, however, to discuss the effect of possible recent contamination on the dates and on the correlation of the layers studied with the climatic sequence in North Western Europe. In fig. 2 the climatic curve for North Western Europe has been given; the chronology is mainly based on our recent dating results (partly unpublished). The climatic variations were derived from pollenstudies by Dr. S. Th. Anderson (Copenhagen) and Dr. W. Zagwijn (Haarlem). It is certainly stimulating for further research if one considers the implications of the Fellabrunn dates for the correlation of the loess stratigraphy with the curve of fig. 2.

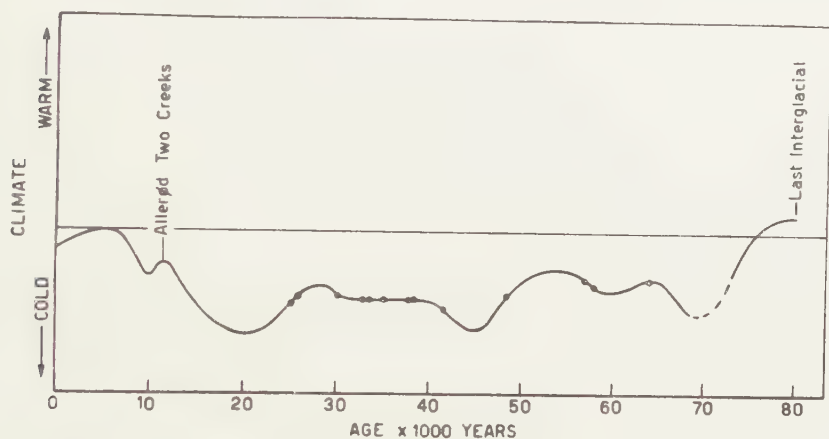


Fig. 2. Climatic curve for North Western Europe (see text).

Assuming that the present dates are not affected by recent contamination, they correlate the black earth's 3-5 with what is called in the dutch pollendiagrams the pleniglacial period around 40 000 years ago. There is apparently no objection against this correlation as far as the climate is concerned. According to Brandtner and Fink (l.c.) these black earth's were formed in a very cool period interrupted by colder intervals in which deposition of loess dominated. Further evidence is provided by the fact that Aurignacien settlements are found in the upper black earth's (or correlated layers in other loess regions). All our Aurignacien dates are around 31 000, which coincides with the date for sample (2) which still contains a considerable amount of humus, though it was not as dark as the black-earth layers (3) and (5).

The age of the upper sample (16 000 years) is not unreasonable. At that time deposition of loess will have been fast, whereas the production of organic material will have been extremely small because of the cold climate. So the age is compatible with the low humus content of the sample.

In the foregoing discussion it was assumed that no recent infiltration occurred. The activity of sample (1) indicates that an eventual correction is smaller than 0.17 min for the deeper samples. Subtracting 0.17 min from the activity of (2), (3) and (5) their ages become 40 000, 41 000 and more than 55 000 years, respectively. Even this maximum correction leaves the upper black earth in the same pleniglacial period, whereas the much higher age of the lower layer indicates that the correction has been too large. Moreover the gap between the Aurignacien dates and the black earth dates becomes too large now.

### *The weathering horizon*

The age of the weathering horizon has been subject of many speculations in the last few years. Unfortunately the sample did not contain enough humus to obtain a date. The minimum age of the overlying black earth (42 000 years) indicates that the weathering ended before that time. Moreover the weathering was followed by a period of deposition of loess before a new soil developed in this loess. Though a line has been drawn in fig. 2 between 45 and 57 000 years, our knowledge of the climate in that period is actually fairly scanty, and we cannot exclude the existence of a separate warmer period about 48 000 years ago. So the weathering could have occurred around 48 000 years ago, or in the period between 67 and 57 000 years ago (fig. 2). It would take us too far here to produce all evidence in support of each of these two possibilities or against it, but 48 000 is perhaps the most probable choice.

### *General conclusions*

The results of the present investigations are promising enough to proof the possibility of dating loess profiles with, or even without, fossile soils. The discussion of eventual recent contamination demonstrates that it is necessary to make first of all a detailed analysis of the humus content over the whole depth in order to make the best choice of the samples, including the samples which provide an upper limit for the amount of infiltrated humus. The latter should contain a small amount of original C<sup>14</sup>; so they should have a low humus content combined with a high age. Needless to say these control samples should overlie the samples which are to be dated in order to be sure that these contain less recent contamination.

### *Acknowledgements*

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